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PRESENT PROBLEMS IN RADIOACTIVITY.*

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SINCE the initial discovery by Becquerel of the spontaneous emission of new types of radiation from uranium, our knowledge of the phenomena exhibited by uranium and the other radioactive bodies has grown with great and ever increasing rapidity, and a very large mass of experimental facts has now been accumulated. It would be impossible within the limits of this article to review even briefly the more important experimental facts connected with the subject and, in addition, such a review is rendered unnecessary by the recent publication of several treatises† in which the main facts of radioactivity have been dealt with in a fairly complete manner.

In the present article an attempt will be made to discuss the more important problems that have arisen during the development of the subject and to indicate what, in the opinion of the writer, are the subjects which will call for further investigation in the immediate future.

Nature of the Radiations.

The characteristic radiations from the radioactive bodies are very complex, and a large amount of investigation has been necessary to

* Address given to the International Congress of Arts and Science, St. Louis, 1904.

† Mme. Curie, 'Thèse presentée à la Faculté des Sciences,' Paris, 1903. H. Becquerel, 'Recherches sur une propriété nouvelle de la matière,' Typographie de Firmin Didot et Cie,' Paris, 1903. E. Rutherford, 'Radioactivity,' Cambridge University Press, 1904. F. Soddy, 'Radioactivity,' Electrician Co., London, 1904.

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isolate the different kinds of rays and to determine their specific characters. The rays from the three most studied radio-elements, uranium, thorium and radium, can be separated into three distinct types, known as the α, β, and γ rays.

The nature of the α and β rays has been deduced from observations of the deflection of the path of the rays by a magnetic and electric field. According to the electromagnetic theory, a radiation which is deflectable by a magnetic or electric field must consist of a flight of charged particles. If the amount of deflection of the rays from their path is measured when both a magnetic and an electric field of known strength are applied, the value of the velocity of the particles and the ratio e/m, of the charge e carried by the particle to its apparent mass m, can be determined. From the direction of the deviation, the sign of the electric charge carried by the particle can be deduced.

Examined in this way, the β rays have been shown to consist of negatively changed particles projected with a velocity approaching that of light. The experiments of Becquerel and Kaufmann have shown that the β rays are identical with the cathode rays produced in a vacuum tube. This relationship has been established by showing that the value of e/m is the same for the two kinds of rays. In both cases the value of e/m has been found to be about 10^7 electromagnetic units, while the corresponding value of e/m for hydrogen atoms set free in the electrolysis of water is 10^4. If the charge on the β particle —or electron, as it has been termed—is the same as that carried by the hydrogen atom, this result shows that the apparent mass of the electron at slow speeds is about 1/1000 of that of the hydrogen atom. The β particles from the radio-elements are expelled with a much greater speed than the cathode ray particles in a vacuum tube. The velocities of the β particles from radium are not all the same, but vary between 10^{10} and 3×10^{10} cms. per second. The swifter particles move with a velocity of at least 95 per cent. of that of light. The emission by radium of electrons with high but different velocities has been utilized by Kaufmann to determine the variation of e/m with speed. He found that the value of e/m decreased with increase of velocity, showing that the apparent mass increased with the speed. By comparison of the experimental results with the mathematical theory of a moving charge, he deduced that the mass of the electrons was in all probability electromagnetic in origin, *i. e.*, the apparent mass could be explained purely in terms of electricity in motion without the necessity of a material nucleus on which the charge was distributed. J. J. Thomson, Heaviside and others have shown that a moving charged sphere increases in apparent mass with the speed and that, for speeds small compared with the velocity of light, the increase of mass $m = 2e^2/3a$ where e is the

charge carried by the body and a the radius of the conducting sphere over which the electricity is distributed. Kaufmann deduced that the value of $e/m = 1.86 \times 10^7$ for electrons of slow velocity. If the mass of the electrons is electrical in origin, it is seen that $a = 10^{-13}$ cms., since the value of $e = 3.4 \times 10^{10}$ electrostatic units. The results of various methods of determination agree in fixing the diameter of an atom as about 10^{-8} cms. The apparent diameter of an electron is thus minute compared with that of the atom itself.

The highest velocity of the radium electrons measured by Kaufmann was, as we have seen, 95 per cent. of the velocity of light. The power that electrons have of penetrating solid matter increases rapidly with the velocity, and some of those expelled from radium are able to penetrate through more than 3 mms. of lead. It is probable that a few of the electrons from radium move with a velocity still greater than the highest value observed by Kaufmann, and it is important to determine the value of e/m and the velocity of such electrons. According to the mathematical theory, the mass of the electron increases rapidly as the speed of light is approached and should be infinitely great when the velocity of light is reached. This leads to the conclusion that no charged body can be made to move with a velocity greater than that of light. This result is of great importance and requires further experimental verification. A close study of the high speed electrons from radium may throw further light on this question.

Only a brief statement of our knowledge of electrons has been given in this paper. A more complete and detailed account of both theory and experiment will be given by my colleague, Dr. Langevin, in his address on 'Physics of the Electron.'

The α Rays.

The β rays are readily deflected by a magnetic field, but a very intense magnetic field is required to deflect appreciably the α rays. The writer showed by the electric method that the rays of radium were deflected both by a magnetic and electric field, and deduced the velocity of projection of the particles and the ratio, e/m, of the charge to the mass. The direction of deflection of the α rays is opposite in sense to that of the β rays. Since the β rays carry a negative charge, the α particles thus behave as if they carried a positive charge. The magnetic deflection of these rays was confirmed by Becquerel and Des Coudres, using the photographic method, while the latter, in addition, showed their deflection in an electric field and deduced the value of the velocity and e/m. The values obtained by Rutherford and Des Coudres were in very good agreement, considering the difficulty of obtaining a measurable deviation.

Observer.	Value of Velocity.	Value of e/m.
Rutherford	2.5×10^9 cms. per sec.	6×10^3 electromagnetic units.
Des Coudres	1.6×10^9 cms. per sec.	6×10^3 electromagnetic units.

Now the value e/m for the hydrogen atom is 10^4. On the assumption that the α particle carries the same charge as the hydrogen atom, this result shows that the apparent mass of the α particle is about twice that of the hydrogen atom. If the α particle consists of any known kind of matter, this result indicates that it is the atom either of hydrogen or of helium. The α particles thus consist of heavy bodies projected with great velocity, whose mass is of the same order of magnitude as the helium atom and at least 2,000 times as great as the apparent mass of the β particle or electron.

If the α particles carry a positive charge, it is to be expected that the particles, falling on a body of sufficient thickness to absorb them, will, under suitable conditions, give it a positive charge, while the substance from which they are projected acquires a negative charge. The corresponding effect has been observed for the β rays. The β particles from radium communicate a negative charge to the body on which they fall, while the radium from which they are emitted acquires a positive charge. This effect has been very strikingly shown by a simple experiment of Strutt. The radium compound, sealed in a small glass tube, the outer surface of which is made conducting, is insulated by a quartz rod. A simple gold leaf electroscope is attached to the bottom of the glass tube, in order to indicate the presence of a charge. The whole apparatus is enclosed in a glass vessel, which is exhausted to a high vacuum, in order to reduce the loss of charge in consequence of the ionization of the gas by the rays. Using a few milligrams of radium bromide, the gold leaf diverges to its full extent in a few minutes and shows a positive charge. The explanation is simple. A large proportion of the negatively charged particles are projected through the glass tube containing the radium and a positive charge is left behind. By allowing the gold leaf, when extended, to touch a conductor connected to earth, the gradual divergence of the leaves and their collapse becomes automatic and will continue, if not indefinitely, at any rate for as long a time as the radium lasts.

When the radium is exposed under similar conditions, but unscreened in order to allow the α particles to escape, no such charging action is observed. This is not due to the equality between the number of positively and negatively charged particles expelled from the radium, for no effect is observed when the radium is temporarily freed from its power of emitting β rays by driving off the emanation by heat. The writer recently attempted to detect the charge carried by the α rays from radium by allowing them to fall on an insulated

plate in a vacuum, but no appreciable charging was observed. The β rays were temporarily got rid of by heating the radium in order to drive off its emanation. There was found to be a strong ionization set up at the surface from which the rays emerged and the surface on which they impinged. The presence of this ionization causes the upper plate to rapidly lose a charge communicated to it. Although this action would mask to some extent the effect to be looked for, a measurable difference should have been obtained under the experimental conditions, if the α rays were expelled with a positive charge; but not the slightest evidence of a charge was observed. I understand that similar negative results have been obtained by other observers.

This apparent absence of charge carried by the α rays is very remarkable and difficult to account for. There is no doubt that the α particles *behave* as if they carried a positive charge, for several observers have shown that the α rays are deflected by a magnetic field. It is interesting to notice, in this connection, that Villard was unable to detect that the 'canal rays' carried a charge. These rays, discovered by Goldstein, are analogous in many respects to the α rays. They are slightly deflected by a magnetic and electric field and behave like positively charged bodies atomic in size. The value of e/m is not a constant, but depends upon the nature of the gas in the tube through which the discharge is passed. The apparent absence of charge on the α particles may possibly be explained on the supposition that a negatively charged particle (an electron) is always projected at the same time as the positively charged particle. Such electrons, if they are present, should be readily bent back to the surface from which they came by the action of a strong magnetic field. It will be of interest to examine whether the charge carried by the α rays can be detected under such conditions.* Another hypothesis, which has some points in its favor, is that the α particles are uncharged at the moment of their expulsion, but, in consequence of their collision with the molecules of matter, lose a negative electron and consequently acquire a positive charge. This point is at present under examination. The question is in a very unsatisfactory state and requires further investigation.

It is remarkable that positive electricity is always associated with matter atomic in size, for no evidence has been obtained of the existence of a positive electron corresponding to the negative electron. This difference between positive and negative electricity is apparently fundamental, and no explanation of it has as yet been forthcoming.

* Recent experiments have confirmed this point of view. By the use of a strong magnetic field, to remove the slow moving electrons, the charge carried by the α rays has been detected both by J. J. Thomson and the writer.

The evidence that the α particles are atomic in size mainly rests on the deflection of the path of the rays in a strong magnetic and electric field. It has, however, been suggested by H. A. Wilson that the α particle may in reality be a 'positive' electron, whose magnitude is minute compared with that of the negative. The electric mass of an electron for slow speeds is equal to $2e^2/3a$. Since there is every reason to believe that the charge carried by the α particle and the electron are the same, in order that the mass of the positive electron should be about 2,000 times that of the negative, it would be necessary to suppose that the radius of the sphere over which the charge is distributed is only 1/2000 of that of the electron, *i. e.*, about 10^{-16} cms. The magnetic and electric deflection would be equally well explained on this view. This hypothesis, while interesting, is too far reaching in its consequences to be accepted before some definite experimental evidence is forthcoming to support it. The evidence at present obtained strongly supports the view that the α particles are in reality projected matter atomic in size. The probability that the α particle is an atom of helium is discussed later.

Becquerel showed that the α rays of polonium were deflected by a magnetic field to about the same extent as the α rays of radium. On account of the feeble activity of thorium and uranium, compared with radium and polonium, it has not been found possible to examine whether the rays emitted by them are deflectable. There is little doubt, however, that the particles of all the radio-elements are projected matter of the same kind (probably helium atoms). The α rays from the different radioactive products differ in their power of penetrating matter in the proportion of about three to one, being greatest for the α rays from the imparted or 'induced' activity of radium and thorium, and least for uranium. This difference is probably mainly due to a variation of the velocity of projection of the α particles in the various cases. The interpretation of results is rendered difficult by our ignorance of the mechanism of absorption of the α rays by matter. Further experiment* on this point is very much required.

It is of importance to settle whether the α particles of radium and polonium have the same ratio e/m. Becquerel states that the amount of curvature of the α rays from polonium in a field of constant strength was the same as for the α rays from radium. This would show that the product of the mass and velocity is the same for the α particles from the two substances. The α rays of polonium, however, certainly have less penetrating power than those of radium, and

* Bragg and Kleeman (*Phil. Mag.*, Dec., 1904) have recently attacked this question and have offered a very satisfactory explanation of the mechanism of the absorption of the α rays by matter.

presumably a smaller velocity of projection. This result would indicate that e/m is different for the α particles of polonium and radium. It is of importance to determine accurately the ratio of e/m and the velocity for the rays penetrating two substances in order to settle this vital point.

The γ Rays.

In addition to the α and β rays, uranium, thorium and radium all emit very penetrating rays, known as γ rays. These rays are about 100 times as penetrating as the β rays and their presence can be detected after passing through several centimeters of lead. Villard, who originally discovered these rays in radium, stated that they were not deflected in a magnetic field, and this result has been confirmed by other observers. Quite recently, Paschen has described some experiments which led him to believe that the γ rays are corpuscular in character, consisting of negatively charged particles (electrons) projected with a velocity very nearly equal to that of light. This conclusion is based on the following evidence: Some pure radium bromide was completely enclosed in a lead envelope 1 cm. thick—a thickness sufficient to completely absorb the ordinary β rays emitted by radium, but which allows about half of the γ rays to escape. The lead envelope was insulated in an exhausted vessel and was found to gain a positive charge. In another experiment, the rays escaping from the lead envelope fell on an insulated metal ring, surrounding it. When the air was exhausted, this outer ring was found to gain a negative charge. These experiments, at first sight, indicate that the γ rays carry with them a negative charge like the β rays. In order to account for the absence of deflection of the path of the γ rays in very strong magnetic or electric fields, it is necessary to suppose that the particles have a very large apparent mass. Paschen supposes that the γ rays are negative electrons like the β rays, but are projected with a velocity so nearly equal to that of light, that their apparent mass is very great.

Some experiments recently made by Mr. Eve, of McGill University, are of great interest in this connection. He found by the electric method that the γ rays set up secondary rays, in all directions, at the surface from which they emerge and also on the surface on which they impinge. These rays are of much less penetrating power than the primary rays and are readily deflected by a magnetic field. The direction of deflection indicated that these secondary rays consisted, for the most part, of negatively charged particles (electrons) projected with sufficient velocity to penetrate through about 1 mm. of lead. In the light of these results, the experiments of Paschen receive a simple explanation without the necessity of assuming that the

γ rays of radium themselves carry a negative charge. The lead envelope in his experiment acquired a positive charge in consequence of the emission of a secondary radiation consisting of negatively charged particles, projected with great velocity from the surface of the lead. The electric charge acquired by the metal ring was due to the absorption of these secondary rays by it, and the diminution of this charge in a magnetic field was due to the ease with which these secondary rays are deflected. It is thus to be expected that the envelope surrounding the radium, whether made of lead or other metal, will always acquire a positive charge, provided the metal is not of sufficient thickness to absorb all the γ rays in their passage through it.

No conclusive evidence has yet been brought forward to show that the γ rays can be deflected either in a magnetic or an electric field. In this, as in other respects, the rays are very analogous to the Röntgen rays.

According to the theory of Stokes, J. J. Thomson and Weichert, Röntgen rays are transverse pulses set up in the ether by the sudden arrest of the motion of the cathode particles on striking an obstacle. The more sudden the stoppage the shorter is the pulse, and the rays, in consequence, have greater power of penetrating matter. In some recent experiments Barkla found that the secondary rays set up by the Röntgen rays, on striking an obstacle, vary in intensity with the orientation of the vacuum tube, showing that the Röntgen rays exhibit the property of one-sidedness or polarization. This is the only evidence so far obtained in direct support of the wave nature of the Röntgen rays.

If Röntgen rays are not set up when the cathode particles are stopped, conversely, it is to be expected that Röntgen rays will be set up when they are suddenly expelled. Now this effect is not observable in an X-ray tube, since the cathode particles acquire most of their velocity, not at the cathode itself, but in passing through the electric field between the cathode and anticathode. It is, however, to be expected theoretically that a type of Röntgen rays should be set up at the sudden expulsion of the β particles from the radio atoms. The rays, too, should be of a very penetrating kind, since not only are the charged particles projected with a speed approaching that of light, but the change of motion must occur in a distance comparable with the diameter of an atom.

On this view the γ rays are a very penetrating type of Röntgen rays, having their origin at the moment of the expulsion of the β particle from the atom. If the β particle is the parent of the γ rays the intensity of the β and γ rays should, under all conditions, be proportional to one another. I have found this to be the case, for the γ rays always accompany the β rays and, in whatever way the β ray

activity varies, the activity measured by the γ rays always varies in the same proportion. Active matter which does not emit β rays does not give rise to γ rays. For example, the radio tellurium of Marckwald, which does not emit β rays, does not give off γ rays.

Certain differences are observed, however, in the ionizing action of γ and X rays. For example, gases and vapors like chlorine, sulphuretted hydrogen, methyl-iodide and chloroform, when exposed to ordinary X rays, show a much greater ionization, compared with air, than is to be expected according to the density law. On the other hand, the relative ionization of these substances by γ rays follows the density law very closely. It seemed likely that this apparent difference between the two types of rays was due mainly to the greater penetrating power of the γ rays. This was confirmed by some recent experiments of Eve, who found that the relative conductivity of gases exposed to very penetrating Röntgen rays from a hard tube approximated in most cases closely to that observed for the γ rays. The vapor of methyl-iodide was an exception, but the difference in this case would probably disappear if X rays could be generated of the same penetrating power as that of the γ rays.

Thus the results so far obtained generally support the view that the γ rays are a type of penetrating X rays. This view is in agreement too with theory, for it is to be expected that very penetrating γ rays will always appear with the β rays.

No evidence of the emission of a type of Röntgen rays is observed from active bodies which emit only α rays. If the α particles are initially projected with a positive charge, such rays are to be expected. Their absence supplies another piece of evidence in support of the view that the α particle is projected without a charge but acquires a positive charge in its passage through matter.*

Emission of Energy by the Radioactive Bodies.

It was early recognized that a very active substance like radium emitted energy at a rapid rate, but the amount of this energy was strikingly shown by the direct measurements of its heating effect made by Curie and Laborde. They found that one gram of radium in radioactive equilibrium emitted about 100 gram calories of heat per hour. A gram of radium would thus emit 876,000 gram calories per year, or over 200 times as much heat as is liberated by the explosion of hydrogen and oxygen to form one gram of water. They showed that the rate of heat emission was the same in solution as in the solid state, and remained constant when once the radium had reached a stage of radioactive equilibrium. Curie and Dewar showed that the

* Recent experiments indicate, however, that the α particles are charged at the moment of this expulsion.

rate of evolution of heat from radium was unaltered by plunging the radium into liquid air or liquid hydrogen.

It seemed probable that the evolution of heat by radium was directly connected with its radioactivity and the experiments of Rutherford and Barnes proved this to be the case. The heating effect of a quantity of radium bromide was first determined. The emanation was then completely driven off by heating the radium, and condensed in a small glass tube by means of liquid air. After removal of the emanation, the heat evolution of the radium in the course of about three hours fell to a minimum corresponding to one quarter of its original value, and then slowly increased again, reaching its original value after an interval of about one month. The heat emission from the emanation tube at first increased with the time, rising to a maximum value about three hours after its introduction. It then slowly decreased according to an exponential law with the time, falling to half value in about four days.

The curve expressing the recovery from its minimum of the heating effect of radium is complementary to the curve expressing the diminution of the heating effect of the emanation tube with time. The curves of decay and recovery agree within the limit of experimental error with the corresponding curves of decay and recovery of the activity of radium when measured by the α rays. Since the minimum activity of radium, measured by the α rays, after the emanation has been removed is only one quarter of the maximum activity, these results indicate that the heating effect of radium is proportional to its activity measured by the α rays. It is not proportional to the activity measured by the β or γ rays, since the β or γ ray activity of radium almost completely disappears some hours after removal of the emanation.

These results have been confirmed by further observations of the distribution of the heat emission between the emanation and the successive products which arise from it. If the emanation is left for several hours in a closed tube, its activity measured by the electric method increases to about twice its initial value. This is due to the 'excited activity' or in other words to the radiations from the active matter deposited on the walls of the tube by the emanation. The activity of this deposit has been very carefully analyzed, and the results show that the matter deposited by the emanation breaks up in three successive and well marked stages. For convenience, these successive products of the emanation will be termed radium A, radium B and radium C. The time T taken for each of these products to be half transformed and the radiations from each product are shown in the following table:

Product.	*T*	Radiations.
Radium,		α rays.
Emanation,	4 days,	α rays.
Radium A,	3 mins.,	α rays.
Radium B,	21 mins.,	no rays.
Radium C,	28 mins.,	α, β and γ rays.

When the emanation has been left in a closed vessel for several hours, the emanation and its successive products reach a stage of approximate radioactive equilibrium, and the heating effect is then a maximum. If the emanation is suddenly removed from the tube by a current of air, the heating effect is then due to radium A, B and C together. On account, however, of the rapidity of the change of radium A (half value in three minutes) it is experimentally very difficult to distinguish between the heating effect of the emanation and that of radium A. The curve of variation with time of the heating effect of the tube after removal of the emanation is very nearly the same as the corresponding curve for the activity measured by the α rays. These results show that each of the products of radium supplies an amount of heat roughly proportional to its α ray activity. Each product loses its heating effect at the same rate as it loses its activity, showing that the mission of heat is directly connected with the radioactive changes. The results indicated that the product, radium B, which does not emit rays, does not supply an amount of heat comparable with the other products. This point is important and requires more direct verification.

Since the heat emission is in all cases nearly proportional to the number of α particles expelled, the question arises whether the bombardment of these particles is sufficient to account for the heating effects observed. The kinetic energy of the α particle can be at once determined, since e/m and v are known.

The following table shows the kinetic energy of the α particle deduced from the measurements of Rutherford and Des Coudres. The third column shows the number of α particles expelled from 1 gram of radium per second on the assumption that the heating effect of radium (100 gram calories per gram per hour) is entirely due to the energy given out by the expelled α particles.

Observer.	Kinetic.	Number of Particles expelled per Second from 1 Gram of Radium.
Rutherford	5.9×10^{-6} ergs	2×10^{11}
Des Coudres	2.5×10^{-6} ergs	5×10^{11}

This hypothesis that the heating effect of radium is due to bombardment of the α particle can be indirectly put to the test in the following way. It seems probable that each atom of radium in breaking up emits one α particle. On the disintegration theory, the residue

of the atom, after the α particle is expelled, is the atom of the emanation, so that each atom of radium gives rise to one atom of the emanation. Let q be the number of atoms in each gram of radium breaking up per second. When a state of radioactive equilibrium is reached the number N of emanation particles present is given by $N = q/\lambda$ where λ is the constant of change of the emanation. Now Ramsay and Soddy deduced from experiment that the volume of the emanation released from 1 gram of radium was about one cubic millimeter at atmospheric pressure and temperature. It has been experimentally deduced that there are 3.6×10^{19} molecules in one cubic centimeter of gas at ordinary pressure and temperature. The emanation obeys Boyle's law and behaves, in all respects, like a heavy gas, and we may in consequence deduce, since $N = 3.6 \times 10^{16}$ and $\lambda = 2.0 \times 10^{-6}$, the value $q = 7.2 \times 10^{10}$. Now the particles expelled from radium in a state of radioactive equilibrium are about equally divided between four substances, viz., the radium itself, the emanation, radium A and radium C. We may thus conclude that the number of α particles expelled per second from 1 gram of radium in radioactive equilibrium is 2.9×10^{11}. The value deduced by this method is intermediate between the values previously obtained (see previous table), on the assumption that the heating effect is entirely due to the α particles.

I think we may conclude from the agreement of these two methods of calculation that the greater portion of the heating effect of radium is a direct result of the bombardment of the expelled α particles, and that, in all probability, about 5×10^{10} atoms of radium break up per second.*

The energy carried off in the form of β and γ rays is small compared with that emitted in the form of α rays. By calculation it can be shown that the average kinetic energy of the β particle is small in comparison with that of the α particle. This is confirmed by comparative measurements of the total ionization produced by the α and β rays, when the energy of the rays is all used up in ionizing the gas, for the total ionization produced by the β rays is small compared with that due to the γ rays. The total ionization produced by the γ rays is about the same as that produced by the β rays, showing that, in all probability, the energy emitted in the form of these two types of radiation is about the same. From the point of view of the energy radiated and of the changes which occur in the radioactive bodies, the α rays thus play a far more important rôle in radioactivity than the β or γ rays. Most of the products which arise from radium and thorium emit only α rays, while the β and γ rays appear only in the last of the series of rapid changes which take place in these bodies.

* By measuring the charge carried by the α rays, the writer (*Nature*, March 2, 1905) has recently deduced that 6.4×10^{10} atoms of radium break up per second.

Since most of the heating effect of radium is due to the α rays, it is to be expected that all radioactive substances which emit them will also emit heat at a rate proportional to their α ray activity. On this view, both uranium and thorium should emit heat at about one millionth the rate of radium. It is of importance to determine directly the heating effect for these substances and also for actinium and radio-tellurium.

According to the disintegration theory, the α particle is expelled as a result of the disintegration of the atom of radioactive matter. While it is to be expected that a greater portion of the energy emitted will be carried off in the form of kinetic energy by the expelled particles, it is also to be expected that some energy will be radiated in consequence of the rearrangement of the components of the system after the violent ejection of one of its parts. No direct measurements have yet been made of the heating effect of the α particles independently of the substance in which they are produced. Experiments of this character would be difficult, but they would throw light on the important question of the division of the radiated energy between the expelled α ray particle and the system from which it arises.

The enormous emission of energy by the radioactive substances is very well illustrated by the case of the radium emanation. The emanation released from 1 gram of radium in radioactive equilibrium emits during its changes an amount of energy corresponding to about 10,000 gram calories. Now Ramsay and Soddy have shown that the volume of this emanation is about 1 cubic millimeter at standard pressure and temperature. One cubic millimeter of the emanation and its product thus emits about 10^7 gram calories. Since 1 c.c. of hydrogen, in uniting with the proportion of oxygen required to form water, emits 3.1 gram calories, it is seen that the emanation emits about three million times as much energy as an equal volume of hydrogen.

It can readily be calculated on the assumption that the atom of the emanation has a mass 100 times that of hydrogen, that I pound of the emanation some time after removal could emit energy at the rate of about 8,000 horse-power. This would fall off in a geometrical progression with the time, but, on an average, the amount of energy emitted during its life corresponds to 50,000 horse-power days. Since the radium is being continuously transformed into emanation, and three-quarters of the total heat emission is due to the emanation and its products, a simple calculation shows that 1 gram of radium must emit during its life about 10^9 gram calories. As we have seen, the heat emission of radium is about equally divided between the radium itself and the three other α ray products which come from it. The heat emitted from each of the other radioactive substances, while their

activity lasts, should be of the same order of magnitude, but in the case of uranium and thorium the present rate of heat emission will probably continue, on an average, for a period of about 1000 million years.

Source of the Energy emitted by the Radioactive Bodies.

There has been considerable difference of opinion in regard to the fundamental question of the origin of the energy spontaneously emitted from the radioactive bodies. Some have considered that the atoms of the radio-elements act as transformers of borrowed energy. The atoms are supposed, in some way, to abstract energy from the surrounding medium and to emit it again in the form of the characteristic radiations. Another theory which has found favor with a number of physicists supposes that the energy is derived from the radio-atoms themselves and is released in consequence of their disintegration. The latter theory involves the conception that the atoms of the radio-elements contain a great store of latent energy, which only manifests itself when the atom breaks up. There is no direct evidence in support of the view that the energy of the radio-elements is derived from external sources, while there is much indirect evidence against it. Some of this evidence will now be considered. There is now no doubt that the α and β rays consist of particles projected with great speed. In order that the α particle may acquire the velocity with which it is expelled, it can be calculated that it would be necessary for it to move freely between two points differing in potential by about five million volts. It is very difficult to imagine any mechanism, which could suddenly impress such an enormous velocity on one of the parts of an atom. It seems much more reasonable to suppose that the α and β particles were originally in rapid motion in the atom and, for some reason, escaped from the atomic system with the velocity they possessed at the instant of their release. There is now undeniable evidence that radioactivity is always accompanied by the production of new kinds of active matter. Some sort of chemical theory is thus required to explain the facts whether the view is taken that the energy is derived from the atom itself or from external sources. The 'external' theory of the origin of the energy was initially advanced to explain only the heat emission of radium. We have seen that this is undoubtedly connected with the expulsion of α particles from the different disintegration products of radium, and that the radium itself only supplies one quarter of the total heat emission, the rest being derived from the emanation and its further products. On such a theory it is necessary to suppose that in radium there are a number of different active substances, whose power of absorbing external energy dies away with the time, at different but definite rates. This still leaves the fundamental difficulty of the origin of these radioactive

products unexplained. Unless there is some unknown source of energy in the medium which the radioactive bodies are capable of absorbing, it is difficult to imagine whence the energy demanded by the external theory can be derived. It certainly can not be from the air itself, for radium gives out heat inside an ice calorimeter. It can not be any type of rays such as the radioactive bodies emit, for the radioactivity of radium, and consequently its heating effect is unaltered by hermetically sealing it in a vessel of lead several inches thick. The evidence, as a whole, is strongly against the theory that the energy is borrowed from external sources and, unless a number of improbable assumptions are made, such a theory is quite inadequate to explain the experimental facts. On the other hand, the disintegration theory, advanced by Rutherford and Soddy, not only offers a satisfactory explanation of the origin of the energy emitted by the radio-elements, but also accounts for the succession of radioactive bodies. On this theory, a definite, small proportion of the atoms of radioactive matter every second become unstable and break up with explosive violence. In most cases, the explosion is accompanied by the expulsion of an α particle; in a few cases by only a β particle, and in others by α and β particles together. On this view, there is at any time present in a radioactive body a proportion of the original matter which is unchanged and the products of the part which has undergone change. In the case of a slowly changing substance like radium, this point of view is in agreement with the observed fact that the spectrum of radium remains unchanged with its age.

The expulsion of an α or β particle, or both, from the atom leaves behind an atom which is lighter than before and which has different chemical and physical properties. This atom in turn becomes unstable and breaks up, and the process, once started, proceeds from stage to stage with a definite and measurable velocity in each case.

The energy radiated is, on this view, derived at the expense of the internal energy of the radio-atoms themselves. It does not contradict the principle of the conservation of energy, for the internal energy of the products of the changes, when the process of change has come to an end, is supposed to be diminished by the amount of energy emitted during the changes. This theory supposes that there is a great store of internal energy in the radio-atoms themselves. This is not in disagreement with the modern views of the electronic constitution of matter, which have been so ably developed by J. J. Thomson, Larmor and Lorentz. A simple calculation shows that the mere concentration of the electric charges, which on the electronic theory are supposed to be contained in an atom, implies a store of energy in the atom so enormous that, in comparison, the large evolution of energy from the radio-element is quite insignificant.

Since the energy emitted from the radio-elements is for the most part kinetic in form, it is necessary to suppose that the α and β particles were originally in rapid motion in the atoms from which they are projected. The disintegration theory supposes that it is the atoms and not the molecules which break up. Such a view is necessary to explain the independence of the rate of disintegration of radioactive matter, of wide variations of temperature, and of the action of chemical and physical agents at our command. This must be conceded if the term atom is used in the ordinary chemical sense. It is, however, probable that the atoms of the radio-elements are in reality complex aggregates of known or unknown kinds of matter, which break up spontaneously. This aggregate behaves like an atom and can not be resolved into simpler forms by external chemical or physical agencies. It breaks up, however, spontaneously with an evolution of energy enormous compared with that released in ordinary chemical changes. This question will be considered later.

The disintegration theory assumes that a small fraction of the atoms break up in unit time, but no definite explanation is, as yet, forthcoming of the causes which lead to this explosive disruption of the atom. The experimental results are equally in agreement with the view that each atom contains within itself the potentiality of its final disruption, or with the view that the disintegration is precipitated by the action of some external cause, that may lead to the disintegration of the atom, in the same way that a detonator is necessary to start certain explosions. The energy set free is, however, not derived from the detonator, but from the substance on which it acts. There is another general view which may possibly lead to an explanation of atomic disruption. If the atom is supposed to consist of electrons or charged bodies in rapid motion, it tends to radiate energy in the form of electromagnetic waves. If an atom is to be permanently stable, the parts of the atom must be so arranged that there is no loss of energy by electromagnetic radiation. J. J. Thomson has investigated certain possible arrangements of electrons in an atom which radiate energy extremely slowly, but which ultimately must break up in consequence of the loss of internal energy. According to present views, it is not such a matter of surprise that atoms do break up as that atoms are so stable as they appear to be. This question of the causes of disintegration is fundamental and no adequate explanation has yet been put forward.

Radioactive Products.

Rutherford and Soddy showed that the radioactivity was always accompanied by the appearance of new types of active matter which possessed physical and chemical properties distinct from the parent

radio-element. The radioactivity of these products is not permanent, but decays according to an exponential law with the time. The activity I_t at any time t is given by $I_t = I_0 e^{-\lambda t}$, where I_0 is the initial activity and λ a constant. Each radioactive product has a definite change constant which distinguishes it from all other products. These products do not arise simultaneously, but in consequence of a succession of changes in the radio-elements; for example, thorium in breaking up gives rise to Th X, which behaves as a solid substance soluble in ammonia. This in turn breaks up and gives rise to a gaseous product, the thorium emanation. The emanation is again unstable and gives rise to another type of matter which behaves as a solid and is deposited on the surface of the vessel containing the emanation. It was found that the results would be quantitatively explained on the assumption that the activity of any product at any time is the measure of the rate of production of the next product. This is to be expected since the activity of any substance is proportional to the number of atoms which break up per second, and, since each atom in breaking up gives rise to one atom of the next product together with α or β particles, or both, the activity of the parent is a measure of the rate of production of the succeeding product.

Of these radioactive products, the radium emanation has been very closely studied on account of its existence in the gaseous state. It has been shown to be produced by radium at a constant rate. The amount of emanation stored up in a given mass of radium reaches a maximum value when the rate of supply of fresh emanation balances the rate of change of the emanation present.

If q be the number of atoms of emanation produced per second by the radium, and N the maximum number present when radioactive equilibrium is reached, then $N = q/\lambda$, where λ is the constant of change of the emanation. This relation has been verified experimentally. The emanation is found to diffuse through air like gas of heavy molecular weight. It is unattacked by chemical reagents and in that respect resembles the inert gases of the argon family. It condenses at a definite temperature $-150°$ C. Its constant of change is unaffected between the limits of temperature of 450° C and $-180°$ C. Since the emanation changes into a non-volatile type of matter which is deposited on the surface of vessels, it was to be expected that the volume of the emanation would decrease according to the same law as it lost its activity. These deductions based on the theory have been confirmed in a striking manner by the experiments of Ramsay and Soddy. The radium emanation was chemically isolated and found to be a gas which obeys Boyle's law. The volume of the emanation observed was of the same order as had been predicted before its separation. The volume was found to decrease with the time according to

the same law as the emanation lost its activity. Ramsay and Collie found that the emanation had a new and definite spectrum similar in some respects to that of the argon group of gases.

There can thus be no doubt that the emanation is a transition substance with remarkable properties. Chemically it behaves like an inert gas and has a definite spectrum and is condensed by cold. But, on the other hand, the gas is not permanent, but disappears, and is changed into other types of matter. It emits during its changes about one million times as much energy as is emitted during any known chemical change.

From the similarity of the behavior of the emanation of thorium and actinium to that of radium, we may safely conclude that these also are new gases which have only a limited life and change into other substances.

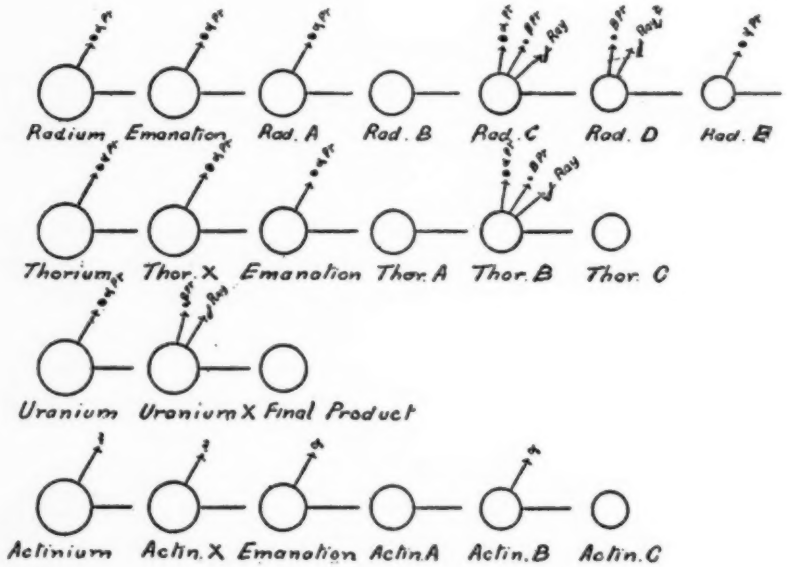


FIG. 1.

The non-volatile products of the radioactive bodies can be dissolved in strong acids and show definite chemical behavior in solution. They can be partially separated by electrolysis and by suitable chemical methods. They can be volatilized by the action of high temperature and their differences in this respect can be utilized to effect in many cases a partial separation of successive products. There can be little doubt that each of these radioactive products is a transition substance possessing, while it lasts, some definite chemical and physical properties which serve to distinguish it from other products and from the parent element.

The radioactive products derived from each radio-element together with the type of radiation emitted during their disintegration, are shown graphically in Fig. 1.

The radiations from actinium have not been so far examined sufficiently closely to determine the character of the radiation emitted by each product. There is some evidence that a product, actinium X, exists in actinium corresponding to Th X in thorium.* It has not, however, been very closely examined.

The question of nomenclature for the successive products is important. The names Ur X, Th X have been retained and also the term emanation. The emanation from the three radio-elements in each case gives rise to a non-volatile type of matter which is deposited on the surface of the bodies. The matter initially deposited from the radium emanation is called radium A. Radium A changes into B and B into C, and so on. A similar nomenclature is applied to the further products of the emanation of thorium and actinium. This notation is simple and elastic and is very useful in mathematical discussion of the theory of successive changes. In the following table a list of the products is given, together with the nature of the radiation and the most marked chemical and physical properties of each product. The time T for each of the products to be half transformed is also added.

The changes which occur in the active deposits from the emanation of radium, thorium and actinium have been difficult to determine on account of their complexity. For example, in the case of radium, the active deposit obtained as a result of a long exposure to the emanation contains quantities of radium A, B and C. The changes occurring in the active deposit of radium have been determined by P. Curie, Danne and the writer. The value of T for the three successive changes is 3, 21 and 28 minutes, respectively. Radium A gives only α rays, B gives out no rays at all, while C gives out α, β and γ rays. These results have been deduced by the comparison of the change of activity with time, with the mathematical theory of successive changes. The variation of the activity with time depends upon whether the activity is measured by the α, β or γ rays. The complicated curves are very completely explained on the hypothesis of three successive changes of the character already mentioned.

The activity of a vessel in which the radium emanation has been stored for some time rapidly falls to a very small fraction after the emanation is withdrawn. However, there always remains a slight residual activity. The writer has recently examined the activity in

* Godlewski (*Nature*, Jan. 19, 1905) has recently separated actinium X. It is similar in chemical properties to thorium X and loses half of its activity in about 10 days.

Radioactive Products.	T.	Rays.	Some Chemical and Physical Properties.
URANIUM.	5×10^8 years.	α	Soluble in excess of ammonium carbonate.
↓			
Uranium X.	22 days.	β, γ	Insoluble in excess of ammonium carbonate.
↓			
Final product.			
THORIUM.	10^9 years.	α	Insoluble in ammonia.
↓			
Thorium X.	4 days.	α	Soluble in ammonia.
↓			
Emanation.	1 minute.	α	Inert gas condenses about —120° C.
↓			
Thorium A.	11 hours.	no rays.	Attaches itself to negative electrode, soluble in strong acids.
↓			
Thorium B.	55 minutes.	α, β, γ	Separable from A by electrolysis.
↓			
Final product.			
ACTINIUM.			
↓			
Actinium X?.			
↓			
Emanation.	3.9 seconds.	α	Gaseous product.
↓			
Actinium A.	41 minutes.	no rays.	Attaches itself to negative electrode, soluble in strong acids.
↓			
Actinium B.	1.5 minutes.	α	Separable from A by electrolysis.
↓			
Final product.			
RADIUM.	1,000 years.	α	
↓			
Emanation.	4 days.	α	Inert gas, condenses —150° C.
↓			
Radium A.	3 minutes.	α	Attaches itself to negative electrode, soluble in strong acids.
↓			
Radium B.	21 minutes.	no rays.	Volatile at 500° C.
↓			
Radium C.	28 minutes.	α, β, γ	Volatile at about 1,100° C.
↓			
Radium D.	About 40 years.	β, γ	Soluble in sulphuric acid.
↓			
Radium E.	about 1 year.	α	Attaches itself to bismuth plate in solution, volatilizes at 1,000° C.
↓			

detail. The residual activity at first mainly consists of β rays, and the activity measured by them does not change appreciably during the period of one year. The α ray activity is at first small, but increases uniformly with the time for the first few months that the activity has been examined. These results receive an explanation on the hypothesis that radium C changes into a product D which emits only β rays. D changes into a product E which emits only α rays. This view has been confirmed by separating the α ray product by dipping bismuth-plate into the solution containing radium D and E. The probable period of these changes can be deduced from observations of

the magnitude of the α and β ray activity at any time. It has been deduced that radium D is probably half transformed in 40 years and radium E is half transformed in about 1 year. The evidence at present obtained points to the conclusion that radium E is the active constituent present in Marckwald's radio-tellurium and probably also in the polonium of Mme. Curie.*

The changes in the active deposit of thorium have been analyzed by the writer, and the corresponding changes in actinium by Miss Brooks.

The occurrence of a 'rayless change' in the active deposits from the emanation of radium, thorium and actinium is of great interest and importance. As these products do not emit either α, β or γ rays, their presence can only be detected by their effect on the amount of the succeeding products. The action of the rayless change is most clearly brought out in the examination of the variation of activity with time of a body exposed for a very short interval in the presence of the emanations of thorium and actinium. Let us consider, for simplicity, the variation of activity with time for thorium. The activity (measured by the α rays) observed at first is very small, but gradually increases with the time, passes through a maximum and finally decays according to an exponential law with the time falling to half value in eleven hours. The shape of this curve can be completely explained on the assumption of the two successive changes, the second of which alone gives out rays. The matter deposited on the body during the short exposure consists almost entirely of thorium A. Thorium A changes into B and the breaking up of B gives rise to the activity measured.

If thorium A does not give out rays, the activity of the body at any time t after removal can be easily shown to be proportional to $e^{-\lambda_2 t} - e^{-\lambda_1 t}$, where λ_1, λ_2 are the constants of change of thorium A and B, respectively. Now the experimental curves of variation of activity are found to be accurately expressed by an equation of this form. A very interesting point arises in settling the values of λ_1, λ_2 corresponding to the two changes. It is seen that the equation is symmetrical in λ_1 and λ_2 and in consequence is unaltered if the values of λ_1 and λ_2 are interchanged. Now the constant of the change is determined by the observation that the activity finally decays to half value in 11 hours. The theoretical and experimental curves are found to coincide if one of the two products is half transformed in 11 hours and the other in 55 minutes. The comparison of the theoretical and experimental curves does not, however, allow us to settle whether

* The writer has recently found that radium E and radio-tellurium have identical rates of decay. Both lose half of their activity in 150 days. This result shows that the active substance in radio-tellurium is a transformation product of radium.

the period of change of thorium A is 55 minutes or 11 hours. In order to settle the point, it is necessary to find some means of separating the products thorium A and B from each other. In the case of thorium, this is done by electrolyzing a solution of thorium. Pegram obtained an active product which decayed according to an exponential law with the time falling to half value in a little less than one hour. This result shows that the radiating product thorium B has the shorter period. In a similar way, by recourse to electrolysis, it has been found that the change actinium B has a period of 1.5 minutes. In the case of radium, P. Curie and Danne utilized the difference in volatility of radium B and C in order to fix the period of the changes.

It is very remarkable that the third successive product of radium, thorium and actinium should not give out rays. It seems probable that these rayless changes are not of so violent a character as the other changes, and consist either of a rearrangement of the components of the atom or of an expulsion of an α or β particle with so slow a velocity that it fails to ionize the gas. The appearance of such changes in radioactive matter suggests the possibility that ordinary matter may also be undergoing slow 'rayless changes,' for such changes can not be detected in the radio-elements unless the succeeding products emit rays.

It is seen that the changes occurring in radium, thorium and actinium are of a very analogous character and indicate that each of these bodies has a very similar atomic constitution.

While there can be no doubt that numerous kinds of radioactive matter with distinct chemical and physical properties are produced in the radio-elements, it is very difficult to obtain direct evidence in some cases that the products are successive and not simultaneous. This is the case for products which have either a very slow or very rapid rate of change compared with the other product. For example, it is difficult to show directly that radium B is the product of radium A and not the direct product of the emanation. In the same way, there is no direct evidence that radium C is the parent of radium D. At the same time, the successive nature of these products is indicated by indirect evidence.

There can be little doubt that each of the radioactive products is a distinct chemical substance and possesses some distinguishing physical or chemical properties. There still remains a large amount of chemical work to be done in comparing and arranging the chemical properties of these products and in determining whether the successive products follow any definite law of variation. The electrolytic method can in many cases be used to find the position of the product in the electrochemical series. The products which change most rapidly are

present in the least quantity in radium and pitchblende. Only the slower changing products like the radium emanation and radium D and E exist in sufficient quantities to be examined by the balance. It is possible that the products radium A, B and C may be obtained in sufficient quantity to obtain their spectrum.

Connection between the α Particles and Helium.

The discovery of Ramsay and Soddy that helium was produced by the radium emanation was one of the greatest interest and importance, and confirmed in a striking manner the disintegration theory of radioactivity, for the possible production of helium from radioactive matter had been predicted on this theory before the experimental evidence was forthcoming. Ramsay and Soddy found that the presence of helium could not be detected in a tube immediately after the introduction of the emanation, but was observed some time afterwards, showing that the helium arose in consequence of a slow change in the emanation itself or in its further products.

The question of the origin of the helium produced by the radium emanation and its connection with the radioactive changes occurring in the emanation is one of the greatest importance. The experimental evidence so far obtained does not suffice to give a definite answer to this question, but suggests the probable explanation. There has been a tendency to assume that helium is the final disintegration product of the radium emanation, *i. e.,* it is the inactive substance which remains when the succession of radioactive changes in the emanation has come to an end. There is no evidence in support of such a conclusion, while there is much indirect evidence against it. It has been shown that the emanation which breaks up undergoes three fairly rapid transformations; but after these changes have occurred, the residual matter—radium D—is still radioactive and breaks up slowly, being half transformed in probably about 40 years. There then occurs a still further change. Taking into account the minute quantity of the radium emanation initially present in the emanation tube, the amount of the final inactive product would be insignificant after the lapse of a few days or even months. Thus it does not seem probable that the helium can be the final product of the radioactive changes. In addition, it has been shown that the α particle behaves like a body of about the same mass as the helium atom. The expulsion of a few α particles from each of the heavy atoms of radium would not diminish the atomic weight of the residue very greatly. The atomic weight of the atoms of radium D and E is in all probability of the order of 200, since the evidence supports the conclusion that each atom expels one α particle at each transformation.

In order to explain the presence of helium, it is necessary to look

to the other inactive products produced during the radioactive changes. The α particles expelled from the radioactive product are themselves non-radioactive. The measurement of the ratio e/m shows that they have an apparent mass intermediate between that of the hydrogen and helium atoms. If the α particles consist of any known kind of matter they must be atoms either of hydrogen or of helium. The actual value of e/m has not yet been determined with an accuracy sufficient to give a definite answer to the question. On account of the very slight curvature of the path of the α particles in a strong magnetic or electric field, accurate determination of e/m is beset with great difficulties. The experimental problem is still further complicated by the fact that the α particles escaping from a mass of radium have not all the same velocity and in consequence it is difficult to draw a definite conclusion from the observed deviation of the complex pencil of rays.

The results so far obtained are not inconsistent with the view that the α particles are helium atoms, and indeed it is difficult to escape from such a conclusion. On such a view, the helium, which is gradually produced in the emanation tube, is due to the collection of α particles expelled during the disintegration of the emanation and its further products. This conclusion is supported by evidence of another character. It is known that thorium minerals like monazite sand contain a large quantity of helium. In this respect they do not differ from uranium minerals which are rich in radium. The only common product of the different radioactive substances is the α particle and the occurrence of helium in all radioactive minerals is most simply explained on the supposition that the α particle is a projected helium atom. This conclusion could be indirectly tested by examining whether helium is produced in other substances besides radium, for example, in actinium and polonium.

The experimental determination of the origin of helium is beset with great difficulty on all sides. If the α particle is a helium atom, the total volume of helium produced in an emanation tube should be three times the initial volume of the emanation present, since the emanation in its rapid changes gives rise to three products each of which emits α particles. This is based on the assumption, which seems to be fulfilled by the experiments, that each atom of each product in breaking up expels one α particle. This at first sight offers a simple experimental means of settling the question, but a difficulty arises in accurately determining the volume of helium produced by a known quantity of the radium emanation. It would be expected that, if the emanation were isolated in a tube and left to stand, the volume of gas in the tube should increase with time in consequence of the liberation of helium. In one case, however, Ramsay and Soddy

observed an exactly opposite result. The volume diminished with time to a small fraction of its original value. This diminution of volume was due to the decomposition of the emanation into a non-gaseous type of matter deposited on the walls of the tube, and followed the law of decrease to be expected in such a case, namely, the volume decreased according to an exponential law with the time falling to half value in four days. The helium produced by the emanation must have been absorbed by the walls of the tube. Such a result is to be expected if the particle is a helium atom, for the α particle is projected with a velocity sufficient to bury itself in the glass to a depth of about 1/100 mm. This buried helium would probably be in part released by the heating of the tube, such as occurs with the strong electric discharge employed in the spectroscopic detection of helium. Ramsay and Soddy have examined the glass tubes in which the emanation had been confined for some time to see if the buried helium was released by heat. In some cases traces of helium were observed.

Accurate measurements of the value of e/m for the α particle and also an accurate determination of the relative volume of the emanation and the helium produced by it would probably definitely settle this fundamental question.

Certain very important consequences follow on the assumption that the α particle is, in all cases, an atom of helium. It has already been shown that the radio-elements are transformed into a succession of new substances, most of which in breaking up emit an α particle. On such a view, the atom of radium, thorium, uranium and actinium must be supposed to be built up in part of helium atoms. In radium, at least five products of the change emit particles, so that the radium atom must contain at least five atoms of helium. In a similar way, the atoms of actinium and thorium (or, if thorium itself be not radioactive, the atom of the active substance present in it) must be compounds of helium. These compounds of helium are not stable, but spontaneously break up into a succession of substances, with an evolution of helium, the disintegration taking place at a definite but different rate at each stage. Such compounds are sharply distinguished in their behavior from the molecular compounds known to chemistry. In the first place, the radioactive compounds disintegrate spontaneously and at a rate that is independent of the physical or chemical forces at our control. Changes of temperature, which exert such a marked influence in altering the rate of molecular reactions, are here almost entirely without influence. But the most striking feature of the disintegration is the expulsion, in most cases, of a product of the change with very great velocity—a result never observed in ordinary chemical reactions. This entails an enormous liberation of energy during the change, the amount, in most cases, being about one million times as great as that observed in any known chemical reac-

tion. In order to account for the expulsion of an α and a β particle with the observed velocities, it is necessary to suppose that the particles are in a state of rapid motion in the system from which they escape. Variation of temperature, in most cases, does not seem to affect the stability of the system.

It is well established that the property of radioactivity is inherent in the radio-atoms, since the activity of any radioactive compound depends only on the amount of the element present and is not affected by chemical treatment. As far as observation has gone, both uranium and radium behave as elements in the usually accepted chemical sense. They spontaneously break up but the rate of their disintegration seems to be, in most cases, quite independent of chemical control. In this respect, the radioactive bodies occupy a unique position. It seems reasonable to suppose that while the radioactive substances behave chemically as elements, they are, in reality, compounds of simpler kinds of matter, held together by much stronger forces than those which exist between the components of ordinary molecular compounds. Apart from the property of radioactivity, the radio-elements do not show any chemical properties to distinguish them from the non-radioactive elements except their very high atomic weight. The above considerations evidently suggest that the heavier inactive elements may also prove to be composite.

Origin of the Radio-elements.

We have seen that the radio-elements are continuously breaking up and giving rise to a succession of new substances. In the case of uranium and thorium, the disintegration proceeds at such a slow rate that in all probability a period of about 1,000 million years would be required before half the matter present is transformed. In the case of radium, however, where the process of disintegration proceeds at over one million times the rate in uranium and thorium, it is to be expected that a measurable proportion of the radium will be transformed in a single year. A quantity of radium left to itself must gradually disappear as such in consequence of its gradual transformation into other substances. This conclusion necessarily follows from the known experimental facts. The radium is being transformed continuously into the emanation which in turn is changed into other types of matter. Since there is no evidence that the process is reversible, all the raduim present must, in the course of time, be transformed into emanation. The rate at which radium is being transformed can be approximately calculated either from the number of α particles expelled per second or from the observed volume of the emanation produced per second. Both methods of calculation agree in showing that in a gram of radium about half a milligram is transformed per year. From analogy with other radioactive changes, it is

to be expected that the rate of change of radium will be always proportional to the amount present. The amount of radium would thus decrease exponentially with the time falling to half value in about 1,000 years. On this view, radium behaves in a similar way to the other known products, the only difference being that its rate of change is slower. We have already seen that, in all probability, the product radium D is half transformed in about 40 years and radium E in about one year. In regard to their rate of change, the two substances radium D and E, which are half transformed in about 40 years and one year respectively occupy an intermediate position between the rapidly changing substances like radium A, B and C and the slowly changing parent substance radium.

If the earth were supposed to have been initially composed of pure radium, the activity 20,000 years later would not be greater than the activity observed in pitchblende to-day. Since there is no doubt that the earth is much older than this, in order to account for the existence of radium at all in the earth, it is necessary to suppose that radium is continuously produced from some other substance or substances. On this view, the present supply of radium represents a condition of approximate equilibrium where the rate of production of fresh radium balances the rate of transformation of the radium already present. In looking for a possible source of radium, it is natural to look to the substances which are always found associated with radium in pitchblende. Uranium and thorium both fulfill the conditions necessary to be a source of radium, for both are found associated with radium and both have a rate of change slow compared with radium. At the present time, uranium seems the most probable source of radium. The activity observed in a good specimen of pitchblende is about what is to be expected if uranium breaks up into radium. If uranium is the parent of radium, it is to be expected that the amount of radium present in different varieties of pitchblende obtained from different sources will always be proportional to the amount of uranium contained in the minerals. The recent experiments of Boltwood, Strutt and McKoy indicate that this is very approximately the case. It is not to be expected that the relation will always be very exact, since it is not improbable, in some cases, that a portion of the active material may be removed from the mineral by the action of percolating water or other chemical agencies. The results so far obtained strongly support the view that radium is a product of the disintegration of uranium. It should be possible to obtain direct evidence on this question by examining whether radium appears in uranium compounds which have been initially freed from radium. On account of the delicacy of the electric test of radium by means of its emanation, the question can be very readily put to experimental trial. This has been done for uranium by Soddy, and for thorium by

the writer, but the results, so far obtained, are negative in character, although if radium were produced at the rate to be expected from theory, it should very readily have been detected.* Such experiments, however, taken over a period of a few months are not decisive, for it is by no means improbable that the parent element may pass through several slow changes, possibly of a 'rayless' character, before it is transformed into radium. In such a case, if these intermediate products are removed by the same chemical process from the parent element, there may be a long period of apparent retardation before the radium appears. The considerations advanced to account for radium apply equally well to actinium, which, in all probability, when isolated will prove to be an element of the same order of activity as radium. The most important problem at present in the study of radioactive minerals is not the attempt to discover and isolate new radioactive substances, but to correlate these already discovered. Some progress has already been made in reducing the number of different radioactive substances and in indicating the origin of some of them. For example, there is no doubt that the 'emanating substance' of Giesel contains the same radioactive substance as the actinium of Debierne. In a similar way, there is very strong evidence that the active constituent in the polonium of Mme. Curie is identical with that in the radio-tellurium of Marckwald. The writer has recently shown that the active constituent in radio-tellurium or polonium is, in all probability, a disintegration product of radium (radium E). The same considerations apply to the radio-lead of Hofmann, which is probably identical with the product radium D. It still remains to be shown whether or not there is any direct family connection between the radioactive substances uranium, thorium, radium and actinium. It seems probable that some at least of these substances will prove to be lineal descendants of a single parent element, in the same way that the radium products are lineal descendants of radium. The subject is capable of direct attack by a combination of physical and chemical methods, and there is every probability that a fairly definite answer will soon be forthcoming.

Radioactivity of the Earth and Atmosphere.

It is now well established, notably by the work of Elster and Geitel, that radioactive matter is widely distributed both in the earth's crust and atmosphere. There is undoubtedly evidence of the presence of the radium emanation in the atmosphere, in spring water, and in air sucked up through the soil. It still remains to be settled whether the observed radioactivity of the earth's crust is due entirely

* In a recent letter to *Nature*, Soddy states that he has found that there is a slow growth of radium in a uranium solution. A similar result has been noted by Whetham.

to slight traces of the known radioactive elements or to new kinds of radioactive matter. It is not improbable that a close examination of the radioactivity of the different soils may lead to the discovery of radioactive substances which are not found in pitchblende or other radioactive minerals. The extraordinary delicacy of the electroscopic test of radioactivity renders it not only possible to detect the presence in inactive matter of extremely minute traces of a radioactive substance, but also in many cases to settle rapidly whether the radioactivity is due to one of the known radio-elements.

The observations of Elster and Geitel render it probable that the radioactivity observed in the atmosphere is due to the presence of radioactive emanations or gases, which are carried to the surface by the escape of underground water. Indeed it is difficult to avoid such a conclusion, since there is no evidence that any of the known constituents of the atmosphere are radioactive. Concurrently with observations of the radioactivity of the atmosphere, experiments have been made on the amount of ionization in the atmosphere itself. It is important to settle what part of this ionization is due to the presence of radioactive matter in the atmosphere. Comparisons of the relative amount of active matter and of the ionization in the atmosphere over land and sea will probably throw light on this important problem.

The wide distribution of radioactive matter in the soils which have so far been examined has raised the question whether the presence of radium and other radioactive matter in the earth, may not, in part at least, be responsible for the internal heat of the earth. It can readily be calculated that the presence of radium (or equivalent amounts of other kinds of radioactive matter) to the extent of about five parts in one hundred million million by mass would supply as much heat to the earth as is lost at present by conduction to its surface. It is certainly significant that, as far as observation has gone, the amount of radioactive matter present in the soil is of this order of magnitude.

The production of helium from radium indirectly suggests a means of calculating the age of the deposits of radioactive minerals. It seems reasonable to suppose that the helium always found associated with radioactive minerals is a product of the decomposition of the radioactive matter present. In the mineral fergusonite, for example, about half of the helium is removed by heating the mineral and the other half by solution. Thus it does not seem likely that much of the helium formed in the mineral escapes from it, so that the amount present represents the quantity produced since its formation. If the rate of the production of helium by radium (or other radioactive substance) is known, the age of the mineral can at once be estimated from the observed volume of helium stored in the mineral and the amount of radium present. All these

factors have, however, not yet been determined with sufficient accuracy to make at present more than a rough estimate of the age of any particular mineral. An estimate of the rate of production of helium by radium has been made by Ramsay and Soddy by an indirect method. It can be deduced from their result that 1 gram of radium produces per year a volume of helium of about 25 cubic mms. at standard pressure and temperature. They, however, consider this to be an under estimate. On the other hand, if the particle is a helium atom, it can be calculated that 1 gram of radium produces per year about 200 cubic mms. of helium.

Let us consider, for example, the mineral fergusonite. Ramsay and Travers have shown that it yields about 1.8 c.c. of helium and contains about 7 per cent. of uranium. It can be deduced from known data that each gram of the mineral contains about one four-millionth of a gram of radium. Supposing that one gram of radium produces $\frac{1}{5}$ c.c. of helium per year, the age of the mineral is readily seen to be about 40 million years. If the above rate of production of helium is an overestimate, the time will be correspondingly longer. I think there is little doubt that, when the data required are known with accuracy, this method can be applied with considerable confidence to determine the age of the radioactive minerals.

Radioactivity of Ordinary Matter.

The property of radioactivity is exhibited to the most marked extent by the radioactive substances found in pitchblende, but it is natural to ask the question whether ordinary matter possesses this property to an appreciable degree. The experiments that have so far been made show conclusively that ordinary matter, if it possesses this property at all, does so to a minute extent compared with uranium. It has been found that all the matter that has so far been examined shows undoubted traces of radioactivity, but it is very difficult to show that the radioactivity observed is not due to a minute trace of known radioactive matter. Even with our extraordinarily delicate methods for the detection of radioactivity, the effects observed are so minute that a definite settlement of the question is experimentally very difficult. J. J. Thomson has recently given an account at the British Association meeting of the work done on this subject in the Cavendish Laboratory, and has brought forward experimental evidence that strongly supports the view that ordinary matter does show specific radioactivity. Different substances were found to give out radiations that differed in quality as well as in quantity. A promising beginning has already been made but a great deal of work still remains to be done before such an important conclusion can be considered to have been definitely established.

THE HARVARD MEDICAL SCHOOL.

By FRANK WALDO, Ph.D.,
CAMBRIDGE, MASS.

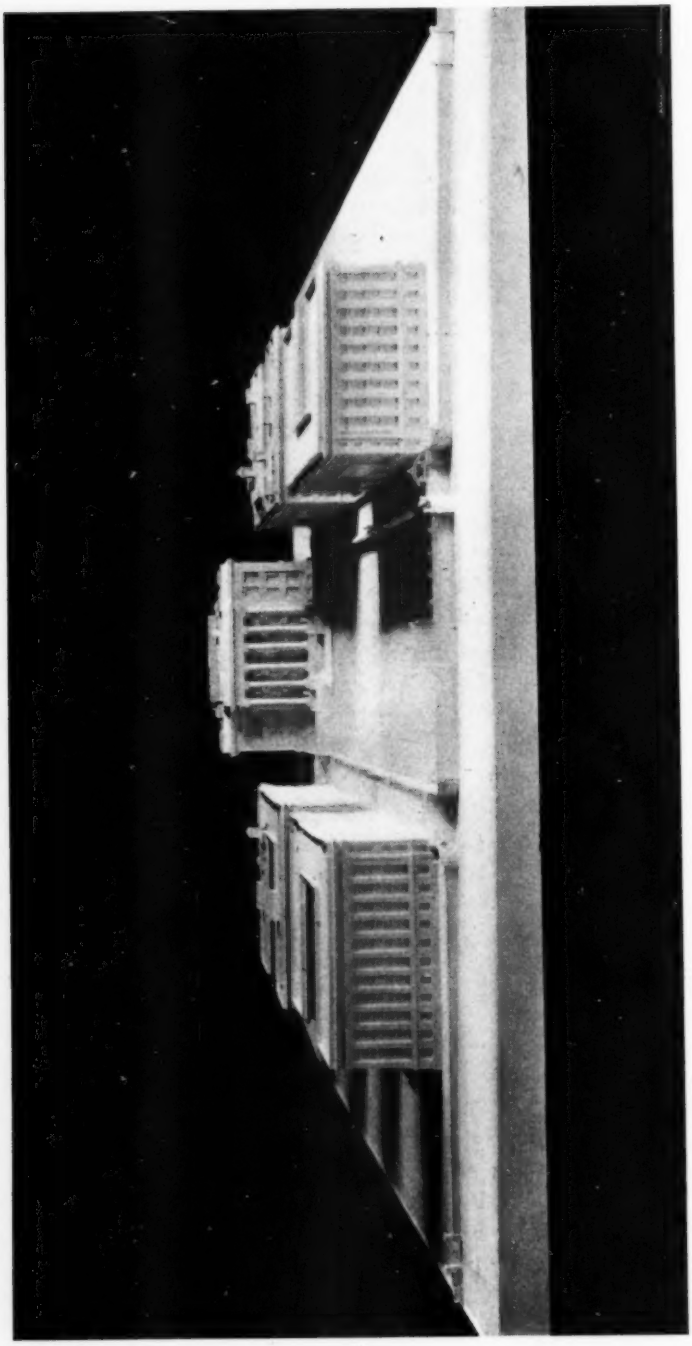
WHILE the new building plant of the Harvard Medical School is approaching completion it seems a fitting time to give a brief account of the work of the school and its equipment. Harvard was the second of the American colleges to establish a school of medicine. The study of medicine in Harvard dates from the close of the war of the American Revolution, when in the years 1782 and 1783 three professorships of medicine were founded; and the first degree, that of M.B. (bachelor of medicine), was conferred in 1788. It was not until 1811 that the degree of M.D. began to be given. Up to 1810 the instruction was given in Cambridge, at which date the school was transferred to Boston, where in 1815 the first medical school building was erected. The second building that was occupied was completed in 1883.

The theory of medicine has of course been taught from the beginning of the Harvard Medical School and eminent men have lectured to its students, but outside hospital and clinic facilities had to be sought. In the first Harvard Medical School building there was no laboratory at all.

With the removal in 1883 to the buildings at present occupied by the school, limited laboratory facilities were provided, in which very important investigations have been conducted. The hospital and clinical service is still, however, so dependent on outside cooperation that this work has been much hampered.

For entrance into the school a college degree is required, or in exceptional cases its equivalent, and since 1892 a four years' course has had to be pursued in order to obtain the degree of doctor of medicine.

The present policy of the school is to so arrange the studies that the student can give his time fixedly for lengthy periods to one subject or group of subjects. Thus anatomy and histology are given the first half of the first year, and physiology and physiological and pathological chemistry during the second half. In the second year pathology and bacteriology are studied during the first half year. It has been the rule to lay down a rigidly required course, throughout, in the study of medicine, but beginning in the fall of the present year the fourth year work will be elected in order to give the student an opportunity to specialize in the department of medicine that he proposes to adopt for his practise.



MODEL OF THE HARVARD MEDICAL SCHOOL.

The Harvard Medical School has numbered among its faculty from the first some of the most eminent physicians of our country. The professors have not been practitioners only, but men of high scientific attainments who have made notable contributions to the science of medicine. Among the most important things accomplished in the recent studies by the Harvard medical faculty may be mentioned discoveries concerning congenital dislocation, cancer, acetonemia, blood pressure, small-pox and scarlet fever.

The contributive activity of various departments of the Harvard Medical School is indicated by the list of publications made during the year from October 1, 1903, to October 1, 1904. Anatomy, 7; physiology, 9; histology and embryology, 3; bacteriology, 7; pharmacology and therapeutics, 2; pathology, 22; comparative pathology, 4; surgery, 14; hygiene, 8. Probably nearly as many more investigations were being carried on but were not published within the period mentioned.

The school is about to enter upon a new and distinct period in its history as the possessor of the finest equipment for medical study of any medical school in the world. In its new location the Harvard Medical School will be enabled to carry on in the most satisfactory manner the study of medicine in theory, practise and laboratory investigation. It, of course, remains to be seen how thoroughly the conditions will be utilized by the faculty and students in furthering the advance of medical knowledge and medical study, but the excellent work done with limited facilities bespeaks a great future activity.

The provisions made in the new buildings for the study of medicine are those that are demanded by the medical knowledge and the advanced methods of the times. Apropos of this it has been well said that the advance in medicine during the past thirty years has been greater than in all preceding time.

The distribution of the buildings, which are being erected at an expenditure of about $2,000,000, and their general style of architecture is shown in the accompanying illustration, from a photograph of a model that was exhibited at the St. Louis Fair, in which the administration building appears in the center at the head of the court, while on the right (facing the picture) the front building is to be devoted to the subjects of hygiene and pharmacology, and the second one to physiology and physiological chemistry. The front building on the left is to be occupied by the departments of surgery, bacteriology and pathology; and the one behind it to anatomy, histology and embryology.

Work upon these new buildings was begun in September, 1903, but it is not expected that they will be completed until towards the close of 1905, which will preclude their being occupied until the fall term of 1906.

The design of the buildings, which are of marble, is distinctly Grecian, and when completed they will form a noteworthy group merely from the architectural point of view. The work of construction is being pushed forward as rapidly as possible. At the time of present writing, the building to be devoted to hygiene and pharmacology is farthest advanced towards completion; the walls are up on the one for surgery, bacteriology and pathology; the iron framework and the walls are in position for the one for physiology and physiological chemistry, and the building for histology and embryology is nearly as far advanced. Only the foundation and a little of the upper portion of the administration building have been erected.

It is of interest to note that much of the funds that have been so generously contributed to enable the Harvard Medical School to make this forward leap is New York money. Mr. J. P. Morgan gave the three buildings at the back of the group, and Mrs. Collis P. Huntington and Mr. David Sears gave the buildings in the foreground. According to the treasurer's report and other accounts, Mr. Morgan gave $1,135,000; Mrs. Huntington, $250,000; Mr. Sears, $250,000, and to these sums must be added a million from Mr. John D. Rockefeller, nearly $371,000 from Henry L. Pierce (1898) and about a half million dollars from other sources.

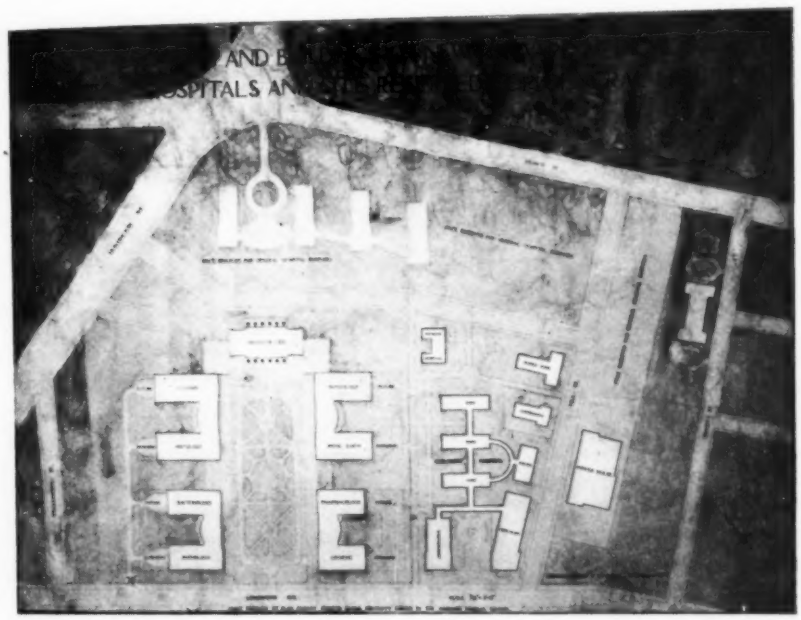
In the erection of such an extended plant for the medical school all possible precautions have been taken to make it suit its purpose in all respects, and to allow for the expansion of the school and for increased demands on the part of medical instruction.

The four main laboratory buildings have each two wings; and not only have the assignments of location for each department been carefully considered as regards the school as a whole, but the allied or supplementary subjects are placed in the wings that are united through a common center. Connecting these wings is an amphitheater over which are placed the special libraries pertaining to the departments occupying the wings. The arrangement of these departments is such as to place in the same building those that are most intimately connected. The actual arrangement adopted has already been outlined in the mention of the various buildings at the beginning of this article. It must be remarked in addition that the study of surgery is provided for in various departments. The arrangement of the wings is such that they may be extended as the school grows so as to ultimately have three-fold the working capacity at present provided for. In the construction of the various buildings and their adaptation to their special purposes, the questions of light, heat and ventilation have been carefully considered; especial use of the principle of lighting by high windows has been made since this insures a good light at the rear of the rooms.

In general, each laboratory wing is divided longitudinally by a broad corridor, and the rooms on each side, which are in most instances of convenient size (23 feet by 30 feet), have adjustable terra cotta walls whereby the rooms may be enlarged or reduced in size according to needs in individual cases. But in the case of the physiological-chemistry building there is no medial corridor, and the laboratories are placed across the wing. Also in the building devoted to pathology and bacteriology one wing contains two large teaching laboratories, while the other wing is divided up into smaller rooms for research work.

In the administration building there are the school offices, a general reading room, an alumni room, four lecture rooms and the Warren Museum occupies the third floor.

The general public associates the names of Pasteur and Koch with single discoveries, but fails to realize that those men have introduced



new methods of work and study, and that the things that their names are especially associated with are but incidents in broad systems; and it is the encouragement of such studies and their practical application that the Harvard Medical School has especially in mind in the arrangement of its new laboratory equipment.

We wonder that the great improvements and discoveries in medicine are not more widely applied. How can they be when the great majority of practitioners have not had the scientific training necessary

to enable them to understand and apply what is being done by the most advanced workers and discoverers? The training that the medical student must undergo in order to enable him to comprehend and apply with intelligence the new methods and discoveries in medicine can not be obtained in a poorly equipped institution nor by poorly equipped minds. It is for this reason that Harvard has hailed with such joy the incoming of the means to equip her medical school properly, and has so raised its standard of admission that the equivalent of a college degree is demanded of those permitted to enter the school.

The study of medicine, broadly considered, has reached such a stage that its present day aspect can be taught only in a great university and by university methods. The old-fashioned medical school served its purpose; but it has had its day, and it can no longer prepare its students to meet the demands of modern medical science.

The new equipment of the medical school will greatly strengthen the connection between it and the college at Cambridge. Hitherto it has been a school apart from the main university, and many a man has graduated from Harvard College without being made practically aware that there is such a thing as the Harvard Medical School. It will be possible under the new conditions to greatly enlarge the scope of the electives that bear on a medical education that may be taken by members of the college or other departments of the university. The departments of psychology and physics in the university can now be properly correlated with the Medical School both in pedagogy and in original investigations.

Hitherto the lack of space facilities and apparatus has made the lecture room and the clinic the main features of student contact with the professors of the school, and this has of necessity kept the feeling of a technical school alive in the student body. The new equipment will incite and foster the growth of the broader university spirit in both study and research. The medical school will now be able to do what it could not do before, that is, to offer facilities to students and investigators of other departments of the university for special study and research under medical school auspices.

One of the most important features of the improved systems of medical study is the learning how to use medical literature and the acquirement of the *habit* of using it. It is only a small proportion of the physicians of the country who can come directly in contact with the special fields of investigation in medicine, and so the chief available channel for keeping up with current progress is through medical literature. The Harvard Medical School possesses unusual facilities for the training of its students in the proper use of the literature of the science. In the first place there are the great general libraries of Boston and Harvard College forming cojointly one of the best col-

lections of books in the world. Next, there is the Boston Medical Library, which is freely open to the Harvard medical students and which possesses one of the most complete collections of medical books in existence, besides containing an unrivaled display of medical journals, which number between seven and eight hundred and embrace the publications of all important countries.

In the various departments of the Harvard Medical School collections of books have been made that serve as technical working libraries; and in the plans for the new buildings this very important feature has been duly provided for. Thus in connection with each laboratory there will be such books, pamphlets, reports and journals as, in the opinion of the person in charge, are the most necessary reference books for students pursuing that specialty. A medical student so trained in the use of medical literature can hardly be content to depend upon antiquated text-book knowledge in his practise in after years.

In that most important matter of applied medicine—hospital service and clinics—the new conditions of the Harvard Medical School promise to be as nearly ideal as the forethought of man can plan. When the grounds for the new site were purchased, enough land was secured to permit the erection of a number of hospital buildings adjacent to the medical school group. Appreciating the advantages of a close connection with the Harvard Medical School, the trustees of several of the new local hospital movements have availed themselves of the opportunity offered and have secured building sites convenient to the school. Moreover, they have signified the intention of joining forces as completely as possible in the carrying on of their humane work. There is first of all the new Brigham Hospital with its foundation of about five millions.

The trustees of the Brigham Hospital fund have signified their intention, after some legal complications have become settled, of purchasing ten acres of the Harvard Medical School grounds as a site for their proposed buildings; but without restriction or accompanying agreement of alliance. Cooperation will mean much to the Harvard Medical School, and quite as much to the hospital. The new Children's Hospital has a location on the west of the medical school buildings, and the Thomas Morgan Rotch Infant's Hospital will build on the school grounds. Near by is the new building of the Samaritan Hospital which was commenced last May; and within easy reach by cars is the Free Hospital for Women. The affiliation of the Harvard Medical School with these institutions will give it the best hospital connections of any medical school in America.

Alpheus Spring Packard

ALPHEUS SPRING PACKARD.

By Professor A. D. Mead,
brown university.

ANOTHER American naturalist of the generation and the type to which belonged Leidy, Cope, Baird, Goode and Hyatt has passed away, Alpheus Spring Packard, professor of zoology and geology at Brown University.

His lineage of sturdy, scholarly men, his academic heritage from great naturalists and the freshness of natural history in America at the time he commenced his career are all perceptible in the sterling quality and the wide range of his life work. The grandfather of the naturalist, the Rev. Dr. Hezekiah Packard, was a revolutionary soldier and fought at Bunker Hill. He received from Harvard College the degrees of A.B., A.M. and D.D. and was an eminent preacher, teacher and writer. The Rev. Dr. Jesse Appleton, one of the early presidents of Bowdoin College, was Professor Packard's maternal grandfather. His father, Alpheus Spring Packard, was a member of the Bowdoin faculty for sixty-five years and served the college successively as tutor, professor of ancient languages and classic literature, of rhetoric and oratory, of natural and revealed religion; as librarian and as acting president. He was an author and a revered teacher; it was of him that Longfellow wrote in his Morituri Salutamus delivered at the celebration of the fiftieth anniversary of the poet's class,

> "they all are gone
> Into the land of Shadows,—all save one,
> Honor and reverence, and the good repute
> That follows faithful service as its fruit,
> Be unto him, whom living we salute."

Professor Packard was born at Brunswick, Maine, February 19, 1839, and died at his home in Providence, February 14, 1905, after an illness of about six weeks. He entered Bowdoin College at the age of eighteen. In his senior year he commenced the serious pursuit of investigations in natural history and continued it with unremitting zeal until the very week of his death, when he insisted upon correcting the proof of his last memoir, published by the National Academy, of which he had long been a distinguished member. While an undergraduate student he enjoyed the friendship and the inspiring instruction of Dr. Paul A. Chadbourne, who was afterwards president of Williams College. It was through him that Packard joined the

Williams College Expedition to Greenland and Labrador in 1860, and to him, many years later, he dedicated his book 'The Labrador Coast,' with grateful acknowledgment of the encouragement and many kindnesses he had received from him in early student days. Three years of graduate study (1861–64) with Louis Agassiz at Cambridge not only brought Packard under the influence of this great naturalist and scientific missionary to America, but brought him naturally into close touch with that older generation of 'lawgivers' who had passed away, but with whom Agassiz had worked; Oken, Humboldt, Cuvier Lamarck and St. Hilaire. The momentum gathered through the labors of these men in what we should call now general natural science was passed on through Agassiz to his many pupils who have rendered such splendid service to natural science in this country, Alexander Agassiz, Hyatt, Packard, Putnam, Morse, Wilder, Brooks, Verrill, Allen, Scudder, Whitman and Jordan.

To these young men American geology and the American fauna, living and extinct, offered extensive and rich choice in fields of research. Packard chose them all and has left his mark upon them all: geology; paleontology; systematic, structural and economic zoology; embryology, and even anthropology. At Cambridge while he was studying with Agassiz he was also pursuing a course in medicine—as Agassiz himself had done in Munich more than thirty years before—and received his S.B. from Harvard and his M.D. from Bowdoin (the Maine Medical School) in the same year, 1864. With respect to theoretical biology Agassiz's laboratory was an interesting place during this period following the publication of Darwin's 'Origin of Species.' Uncritical acceptance of the doctrine of organic evolution would have been impossible in view of Agassiz's attitude toward this 'notion . . . ever returning upon us with hydra-headed tenacity of life, and presenting itself under a new form as soon as the preceding one has been exploded and set aside . . . .' The theoretical phase of biology appealed strongly to Packard and to it he devoted much time and study, especially after the year 1870. He was an ardent admirer of Lamarck, adopted many of his ideas and applied them to new material; and with Cope and Hyatt founded the school of evolutionary thought for which he proposed the name Neo-Lamarckian. To all these subjects, except medicine, Packard contributed papers, memoirs, general books or text-books, and upon them all, except medicine, he lectured to his classes in Brown University.

His geological researches began with his student trip to Labrador in 1860. From these and later studies in Labrador (1864) several articles resulted; among them were the 'Glacial Phenomena of Maine and Labrador' (1866) and the book already referred to, 'The Labrador Coast' (1891). While assistant on the Maine Geological Survey

1861–62, he made discoveries of fossils in the Fish River Region which determined the age of these rocks. About five years later, 1867, he discovered the glacial striæ radiating from Mt. Washington. In 1867 he published a 'Revision of the Fossorial Hymenoptera of North America' and in 1882 the text-book 'First Lessons in Geology.' Many other geological papers have come from his pen.

His zoological articles, especially those on insects, far outnumbered those upon other subjects. Professor Samuel Henshaw in 'The Entomological writings of Alpheus Spring Packard' enumerates three hundred and thirty-nine papers, books and notes published up to 1887; among them the 'Monograph of the Geometrid Moths' 1876, the text-books, 'A Guide to the Study of Insects' 1869, which ran through eight editions in the next fifteen years, 'Insects of the West' 1877, 'Our Common Insects' 1876, 'Half Hours with Insects' 1877. Many important works upon insects have come from his pen since that date. These include the well-known monograph on the Bombycine Moths, 1895, and the text-books 'Entomology for Beginners' 1888, 'Forest and Shade Tree Insects' 1888, and the 'Text-Book of Entomology' 1898. During the last year three insect articles were completed. One of these, his last paper, is a large 'Monograph of the Bombycine Moths of America, including their Transformations and Origin of the Larval Markings and Armature' which will appear as a Memoir of the National Academy of Sciences.

Professor Packard was known through many articles and books on zoological subjects outside the field of entomology. 'The Development and Anatomy of Limulus Polyphemus' 1871, 'The Monograph of North American Phyllopod Crustacea' 1883, the 'Life History of Animals, including Man, or Outlines of Comparative Embryology' 1876, the 'Zoology for Students and General Readers' 1879, 'The Cave Fauna of North America' 1888, are some of the books which brought to naturalists and students new data or new arrangements and treatment of subjects which were highly appreciated. To-day, amid the profusion of newer text-books, it is not easy to accord to these older works their full merit or to realize their true value. Regarding the 'Life History of Animals,' Kingsley says it 'was the first attempt since the day of Agassiz's Lowell Institute Lectures to summarize the facts of Embryology'; and of the 'Zoology' he says, it 'was the first attempt to give American students a truly scientific text-book in which morphology and classification were given equal prominence.'

Anthropological and ethnological investigations he followed with keen interest, and contributed to these subjects several miscellaneous notes and papers.

Of his general books that on 'Lamarck, the Founder of Evolution, His Life and Work,' 1901, is especially noteworthy. The work was

the result of years of study, interpretation and defense of Lamarck's writing, and of a journey to France for the purpose of looking up the records of Lamarck's private and professional life. In this portrayal of Lamarck there may be perceived something of the personal loyalty, esteem and almost affectionate regard which Professor Packard unconsciously but inevitably showed in conversing about the 'founder of evolution.' These conversations about Lamarck and other naturalists of the past were one of the many things which revealed to Packard's friends his own unconscious genuineness. The absolute absorption of his interests by natural science made the persons, events and facts connected with it as real and natural as each day's life.

The quiet reticence which characterized Packard did not grow out of a life of contemplation without action and contact with things and men. From his student days he had actively participated in expeditions, commissions, surveys, foreign and domestic travel and in the founding of scientific institutions. At the end of his junior year in college, he went with Chadbourne on the Greenland and Labrador Expedition. In his senior year he went with a class on a trip to the Bay of Fundy. Immediately after his graduation, in 1861–62, he explored the wilderness of northern Maine as assistant on the Geological Survey of that state. He enlisted as assistant surgeon in the First Maine Veteran Volunteers which joined the Army of the Potomac in 1864. He joined a party organized by a Mr. William Bradford in the summer of 1864 and paid a second visit to Labrador. In 1867 he was examining the glacial traces in the White Mountains. The animals of the Florida reefs and of the Beaufort, N. C., flats engaged his attention in 1869–70, and the next year we find him studying the development of crustacea and collecting fossil mollusca in Charleston, S. C. In 1871 he was appointed Massachusetts state entomologist, a position which he held three years. Meanwhile he visited Europe and spent much time in the study of insects at the British Museum. The summer of 1873 found him working again with Agassiz, as a teacher in the Anderson School of Natural History at Penikese. He returned the next year to the school and for a time was the dean of its faculty. He held an appointment from 1875 to 1877 as one of the zoologists on the United States Geological Survey under Ferdinand V. Hayden, and from 1877 to 1882 was a member of the United States Entomological Commission. In the latter capacity he made extensive trips in the western part of the United States, investigating the breeding grounds and distribution of the Rocky Mountain locust. In 1874 he was appointed on the Kentucky Geological Survey, and with Putnam made explorations of the great caves and observations upon the cave fauna which laid the foundations of his later works upon these subjects. He visited Mexico in 1885, Cuba in

1886, and in 1889 traveled in Morocco, Algeria, Egypt and Europe, arriving in Paris to attend the meetings of the International Zoological Congress of which he was elected an honorary president.

In the course of his long and active career he was associated with many American institutions and took a prominent part in the founding of some of them. After his service in the army he became librarian and acting custodian of the Boston Society of Natural History 1865–66. Then with his friends, Hyatt, Morse, Putnam and Cooke, he accepted a position in the Essex Institute in Salem which was at that time a thriving and important scientific institution. When the Peabody Academy of Science in Salem was founded in 1868 and absorbed the Essex Institute, Packard became its curator of Invertebrates and later, in 1876, was elected director of the academy. It was this group of fellow students, Packard, Hyatt, Morse and Putnam, who in 1868 founded the *American Naturalist,* and Packard remained its editor-in-chief for twenty years. While curator at the Peabody Institute, he lectured on entomology in the Massachusetts State College, 1869–77, in the Maine Agricultural College, 1871, and on natural history in Bowdoin College, 1871–74.

Packard was also prominently connected with a novel undertaking which has proved to be of inestimable value in the development of biological science in America. The Anderson School of Natural History at Penikese Island was inaugurated by Louis Agassiz in 1873; here Packard taught for two years and then, when this school was given up on account of Agassiz's death, Packard perpetuated the idea by establishing a summer school of biology at Salem under the auspices of the Peabody Academy. He directed this laboratory until 1878, when he left Salem to accept the professorship of zoology and geology at Brown University. The cherished idea of the seaside laboratory of natural history then took form in the Annisquam laboratory established through the efforts of Packard's colleague Professor Hyatt, under the auspices of the Woman's Education Society of Boston, and this experiment in turn led the way to the establishment of the Marine Biological Laboratory at Woods Hole. This institution, directed for so many years by a Penikese student, Dr. C. O. Whitman, and the United States Fish Commission laboratory at Woods Hole, established by Professor Spencer F. Baird, a teacher at the Penikese school, have not only afforded inspiration and opportunity for research to hundreds of biologists, but have given birth to scores of similar laboratories on the Atlantic and Pacific coasts.

From the colleges in which he was a student Packard received the degrees of A.B., Bowdoin, 1861; A.M., Bowdoin, 1862; M.D., Bowdoin, 1864; S.B., Harvard, 1864; Ph.D., Bowdoin, 1879; LL.D., Bowdoin, 1891.

The value of Packard's work can not be best estimated and, perhaps, is not fully appreciated by the younger generation of morphologists and physiologists, whose energies are absorbed in the amazing elaboration of cell studies; it must be left to 'the judgment of his confrères.' From these men of many countries have come unequivocal tokens of approval. The American Academy of Sciences elected him to membership in 1872, the Société Royale des Sciences de Liège, Belgium, in 1875; the Society of Friends of Natural Science in Moscow, in 1891. In 1901 he was elected a foreign member of the Linnean Society of London. In this distinction he once more renewed the comradeship of his fellow students and collaborators at Penikese, Alexander Agassiz and C. O. Whitman, who were the only other American zoological members. He was elected to membership in the entomological societies of London, Paris, St. Petersburg, Stockholm and Brussels; was one of the honorary presidents of the International Zoological Congress at Paris, and honorary president of the Zoological Section of the French Association for the Advancement of Science, and in 1898 was vice-president of the Zoological Section of the American Association for the Advancement of Science.

In the latter years of his teaching, his colleagues and the students of the university where for twenty-six years he had held a professorship awarded him tokens of esteem rarely bestowed upon their colleagues and teachers. The members of the faculty constrained him to attend a banquet held in his honor, at which the address of his lifelong friend, Professor Hyatt, completed his modest confusion. A loving cup recently presented to him by his class in zoology was valued by the distinguished, genial teacher above the diplomas of many learned societies.

Professor Packard was not quite sixty-six years of age, but his active scientific career extended over a period of forty-five years, and during that time he published upwards of four hundred books and papers. He married, in 1867, Elizabeth Debby, daughter of the late Samuel B. Walcott, of Salem, who, with two daughters and a son, survives him.

ORGANIZATION IN SCIENTIFIC RESEARCH.

By Professor WM. E. RITTER,
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PROGRESS in science leads to ever greater, more multifarious minutiæ of knowledge, and at the same time to ever clearer revelation of the close and vital interdependence among the different sciences. This characteristic of progress tends inevitably, for the individual investigator, toward an unyielding paradox. On the one hand, he is confronted by an ever increasing mass of detail, which necessitates ever narrower specialization, while, on the other hand, he is required to fit himself ever more thoroughly in an increasing number of sciences. See how it is faring with the zoologist, for example, since his case happens to be one of painful concreteness to the present writer. To enter this field by any of its numerous gateways with fair prospects of being able to achieve much, one needs to be armed to the teeth with weapons obtained in several other fields. In the first place, he ought to be a physiologist among physiologists, with all that implies of physics and chemistry. It is not enough that he be a zoologist with 'pretty fair training in physiology.' In the second place, he ought to handle the mathematician's weapons just as the mathematician himself handles them. Further, he can hardly get on without being geologist, oceanographer, meteorologist, one or another, depending on what aspect of zoology may be his chief interest. For the strict individualist in research it looks as though some of the sciences are in a way to progress themselves to a standstill before long.

What is to be done about it?

It is becoming more and more obvious that in some of the sciences continued progress, particularly in certain directions, calls for the helping hand of workmen whose training and interests are not primarily in the science directly concerned, but in neighboring sciences. It is no longer possible for an investigator engaged upon some of the problems of science, however broad and thorough be his training in sciences other than his own, to use the tools borrowed from other fields with real effectiveness in his own field. It is a question not merely of preliminary training, but as well of *point of view,* to be reached only by continuous and long continued living in a particular realm of knowledge until a certain habit of mind peculiar to that realm has been acquired. This is the sort of help that every science, probably, certainly most sciences, must have from its neighbors. There

are aspects of certain biological problems, for instance, that admit of no other than mathematical treatment; and this treatment ought to be, indeed to be thoroughly sound must be, at the hand not of a botanist or zoologist with some incidental mathematical training, but of a mathematician. The astrophysicist is really a physicist using his tools in astronomy. The biochemist is primarily a chemist applying his cunning in the domain of living things. The paleontologist, whatever else he may be, must be a zoologist, and so on. So important is this matter that at the risk of seeming verbosity, I venture to illustrate it yet further.

Take anthropology, and to make the case more concrete, consider the question of the native peoples of western North America, for in a remote, amateurish sort of way I have an interest in some phases of this question. The broadest, fullest knowledge possible about these tribes is, of course, the ultimate aim. The investigations must then comprehend their somatology, their language, their psychology, their culture and their archeology, which runs into paleontology and so into geology. Now who but one soaked in linguistics is really competent to handle the language end of the problem? But where, think you, is the man thus prepared who is equally soaked in comparative anatomy and thus made equally fit for the somatological end of the problem? He simply does not exist, nor can he.

Or again, take the field of marine biology, about which I speak with some of the confidence that experience is capable of begetting. And first let the distinction be sharply made, as it surely must be, between *marine biology* and *general biology advanced by researches on marine organisms*. The former has for its aim, in the large, the getting of as comprehensive an understanding as possible of the life of the sea. It, of course, presents itself under a great variety of secondary, tertiary, etc., questions; but the sum total of the phenomena of *marine* plants and animals will never be lost sight of as its real aim. The latter makes use of animals and plants that live in the sea in general biological researches. That these organisms happen to be marine is an incident merely. The investigator turns away from his sea organisms without hesitation when others, from whatever source, come to hand that suit his purpose better. Further, the user of marine organisms in such general investigations is quite indifferent to every thing concerning them that does not bear upon his particular problem. He puts aside the marine animal after it has served his purpose without having even noticed, perhaps, the major part of its traits and qualities, and questions about it.

Now marine biology as here comprehended must have the correlated efforts of highly trained investigators in several widely separated fields of science. In the first place, there must be, of course, for the

biological investigations proper, a considerable number of specialists in botany and zoology. Then, in addition, there must be at least the physicist or physical chemist for the physico-chemical study of the water; the geologist for bottom and shore topography and bottom deposits; and the hydrographer must be called in for currents, tides, up-welling water and meteorological conditions.

It is obvious, then, that increased coordination of effort would be distinctly advantageous in many fields of science; and that in some, at least, it is a *sine qua non* to considerable progress in the future. How is this to be brought about? How are these diversities of talent and training to be brought together and held together to the end that they may accomplish that which in no other way can be accomplished? Cooperation among individual workers, entered into on their own initiative and held together by their own cohesiveness, has done something, and probably in future will do more. In some districts of nature, and in some sorts of problems, this may fill the bill. To other districts and other kinds of problems, however, among which are undoubtedly some of the largest and potentially richest, I believe this kind of coordination can not extend in great effectiveness.

Organization around single large problems, or groups of closely related problems, with the two binding elements of talent for organizing and directing, and money for sustaining, I believe to be the direction in which we must look in the future more than we have been looking hitherto.

Something of the value and possibilities of organization in research are usefully illustrated by certain of the science departments of our national government. Particularly to be mentioned is the Geological Survey; and some of the divisions of the Department of Agriculture are likewise notable. But astronomy is giving us object lessons most to the point in this matter. The astronomical observatory with its permanent staff of investigators, each of whose work has a definite bearing upon a common purpose of the observatory, all under the coordinating hand of the director, shows us in principle how much of the scientific work of time to come must be done. It would appear that these splendid instrumentalities of research are getting right at the kernel of the question of means; and it seems as though other sciences ought to be profiting more than they are by the example.

It is likely to be replied that the observatories are made possible solely by the great sums of money given them, and that astronomy is doing no more in the way of equipping herself and organizing her forces than other sciences would do were they equally fortunate in getting money. True, astronomy is, or thus far has been, the favored sister in the family of sciences with those who have riches and a disposition to use them for the promotion of knowledge; and it is said

sometimes that she is thus favored because she is more beautiful, more winsome, than her sisters. Well, possibly this is true; it may be or it may not be. We need not stop to discuss relative merits in this particular, since it is quite aside from the question. If other sciences must do what astronomy is doing in order to get on, and if money in more abundance is essential for this, then more money must be secured by those needing it. If astronomy, by reason of greater winsomeness, is able to get it more easily, that is her good fortune. So far as the essential matter is concerned, it can be only a question of overcoming greater difficulties by greater effort. But the real things are *merit* and *need*. When these *both* are, first, strongly felt, and, second, strongly presented, they are pretty sure to have a relaxing effect upon purse strings somewhere sooner or later, particularly in our country where wealth is so abundant and the general spirit of giving for the promotion of learning so much abroad.

Some of the practical bearings that a wider application of the principle of organization in research would have may now be briefly noticed.

In the first place, as touching the status of research in the universities, there can be no doubt that were research a primary rather than an incidental matter with the scientific departments of the universities, the principle could be applied, without specially greater expenditure of money, to an extent quite impossible under the present order. Supposing, for example, the department of botany of University X were to be organized and equipped primarily with reference to a comprehensive botanical survey of the particularly interesting botanical region in which it may happen to be located; how vastly greater would be its efficiency in forwarding botanical science than if its composition were determined by some other consideration, say the needs of instruction, and the botanical survey were to take its chance. Or again, suppose the department of geology of University Y, situated in a region especially favorable for investigations in dynamic geology, were to be organized primarily with reference to such investigation. A department thus constituted would promote this aspect of geology with a degree of certainty and efficiency quite out of question under prevailing conditions. A point of importance should here be noted relative to what the organizing of a university department on such lines as indicated could mean. Such a department of geology, for example, need not consist merely of the geologists that would constitute the ordinary teaching department of geology, but it would contain for the special needs of the investigations, and hence selected and compensated with reference to this end, persons belonging primarily to other fields of science, and hence presumably to other university departments; and there would be no reason why these should not be members of other

universities at the same time. For example, the department of geology would have need for, let us say, a chemist, and a paleontologist, and these might be employed for the special service, while they could still belong primarily to their respective departments, either of the same or of another university.

The inquiry what effect such a system widely in vogue would have on the individuality of workers in science is quite to the point. Some, I fancy, would be apprehensive lest it should prove harmful to this quality, of right so highly prized among men of science. I am persuaded, however, that a little reflection will show that while individuality might now and then suffer, it would be the gainer in far larger measure. On the whole, instead of being a heavy hand on independent, individual effort, it would furnish opportunity and incentive to it. For observe, it would be a system not of forcing investigators into work, for which they should have neither taste nor fitness, but rather of *selecting* them for tasks for which they would have both, and then of giving them facilities and opportunities for following their bent that they could not generally secure through their individual, unaided efforts.

How familiar a thing it is to all university men of science to see a young man receive his doctor's degree as the reward of some research carried to results with real zeal and talent; but then, under stress of the necessity of earning a livelihood, carried off into some position that affords little or none of either time, facility or incentive for further prosecution of his central interest in science.

There can be no doubt that science, in this country particularly, has been and is a heavy loser from diversion and misapplication of its forces, through the regimen under which research has to be prosecuted. This loss is partly direct and absolute, and partly indirect, through the detached and fragmentary character of what is done under the prevalent highly individualistic order. Of course, it would be idle to contend that this order is wholly bad, or that all that is bad about it could be corrected by such a system. That, however, coordination and concentration through organization would be of even more advantage in several sciences where now practically nothing of the sort exists, there can be no doubt. Such a system largely in operation would undoubtedly work radical change in the means and methods of research in numerous directions, but these need not be discussed here. One further matter only do I mention. That is the question of the effect it would have on instruction in science in the universities and colleges. It is easy to see how to some extent instruction might suffer. It is still easier to see how, on the other hand, it would be a very great gainer; and there can be hardly a question that the gain would far outweigh the loss.

THE ORIGIN OF SPECIES THROUGH SELECTION CONTRASTED WITH THEIR ORIGIN THROUGH THE APPEARANCE OF DEFINITE VARIATIONS.

By Professor T. H. MORGAN,
COLUMBIA UNIVERSITY.

IT is a point of some interest that at the present time those zoologists and botanists, who seem willing to transfer their allegiance from Darwin's theory of natural selection to the theory of the survival of mutations, often insist that the two points of view differ, after all, only in degree and that selection is still the key note to the situation. It seems to me, on the contrary, that there is a fundamental difference between these two views, and in the hope of making this clearer I have attempted in the following pages to contrast in certain respects the applications that have been made of Darwin's theory with the implications of the newer theory of the survival of definite variations.

Attention has seldom, I believe, been called to the fact that only those theories that have been advanced to account for the evolution of animals and plants, have received wide recognition, that pretend to explain how the adaptation of the organism to its surroundings has been acquired. No such requirement is made in the case of theories of evolution of the inorganic world. On what does this difference depend? Why do we make certain demands in the case of organic evolution that we do not make for the evolution of inorganic nature? The answer in part is, that a living thing is unstable, it is easily destroyed, and it must, if it is to maintain its integrity, be able to respond to changes in the outer world in such a way as to keep the balance that makes its existence possible.

It is true that certain chemical substances are also highly unstable, but we find in them no adjustments, no regulations, for maintaining themselves, such as animals and plants exhibit. No theory, as I have said, that pretends to account for the evolution of new organisms, has been regarded as satisfactory unless it explained how the new forms acquire those adjustments that make their life possible. How perfect these adjustments must be is a question that has never been sufficiently considered, partly because of the difficulties surrounding such an examination, and partly because of the widespread belief that living things are as perfectly adapted to their environment as we can imagine possible by the adjustment of their individual parts to the surroundings

in which they live. As an illustration of how such adjustments are supposed to occur I shall take a single example, from Weismann's writings, although it would be an easy matter to give endless examples, similar to this one, from the writings of other Darwinians, who, as a rule, have been only too prone to make use of the same argument in accounting for the origin of the adaptations of animals and plants.

Length of Life as an Adaptation.

Weismann has written a most interesting essay on 'The Duration of Life,' in which he attempts to show that the length of life of the individuals of a species has been regulated by natural selection of individual variations. While it may be granted that in many respects Weismann has out-Darwined Darwin, yet his method in this instance is the same as that which the Darwinians have always employed whenever an occasion occurred.

It has often been pointed out that the life of larger animals is longer than that of smaller ones, and this seems not unreasonable, since in many cases it takes a longer time for a larger animal to reach maturity; yet this relation is by no means universal, as Weismann points out, for, while it is true that an elephant may live two hundred years and a horse not more than forty, yet a man lives longer than a horse, and a cat and a toad may also live forty years. A pig may live no longer than a crayfish. Flourens tried to show that the length of life of an animal is about five times its growing period, but this does not generally hold, since a horse may live ten times as long as it takes to reach maturity.

Complexity of structure can not explain the result, because some very simple forms live to a great age. Weismann also concludes that the length of life is not determined by the 'constitution' of the animal, for, while a queen bee may live for several years, the male lives for only a few weeks. Therefore, Weismann concludes, it is 'proved that physiological considerations alone can not determine the duration of life.'

Thus by an apparent process of exclusion Weismann reaches the conclusion, 'that duration of life is really dependent upon adaptation to external conditions, that its length, whether longer or shorter, is governed by the needs of the species.' In support of this view he points out that 'life does not greatly outlast the period of reproduction except in those species that tend their young, and as a matter of fact we find that this is the case.' How then has this regulation been brought about? Weismann's answer is that the duration depends first on the time required to reach maturity, and since the longer this time the more the chance for destruction, the number of descendents produced must be greater in proportion as the duration of the reproductive period

becomes longer. This statement is obviously of no value, for a sea urchin that may grow up in a year, produces millions of eggs, while an elephant that takes thirty years to mature produces only about six young in the whole course of its life. Equally valueless, it appears to me, is Weismann's statement 'that nature does not tend to secure the longest possible life to the adult individual, but, on the contrary, tends to shorten the period of reproduction as far as possible, and with this the duration of life.'

Coming now to the main question as to how natural selection may be supposed to increase the length of life, Weismann states, that "Duration of life like every other characteristic of the organism is subject to individual fluctuation. From our experience with the human species we know that long life is hereditary. As soon as the long lived individuals of a species obtain some advantage in the struggle for existence they will probably become dominant, and those with the shortest lives will be exterminated."

Without attempting to offer an elaborate refutation of Weismann's view, I should like briefly to present the following considerations:

I. It is, of course, almost self-evident that the existence of a species is closely bound up with its powers of reproduction, but it does not follow from this that the length of life has been adjusted to come to an end when the animal can no longer reproduce itself, *because* it can not reproduce itself any longer.

2. On the contrary it is more probable that the same causes that have led to the cessation of the powers of reproduction may be closely associated with those that bring about a decline in the general vitality; so that while death may follow at a variable period after the power of reproduction is lost, the two processes have not been adjusted to each other by some external need, but are part of the same physiological decline.

3. In the higher animals especially, there may be thousands of immature eggs when the animal ceases to reproduce, as in the case of the human species. It would seem to be greatly to the advantage of a species to have the individual that has surmounted the dangers of youth bring all of its eggs to maturity before it dies; yet such is not the case. The eggs fail to mature not because it is to the advantage or disadvantage of the species to perish after it has set free a part of its eggs, but because the general decline of the organism brings to an end the power to ripen its eggs.

4. The natural duration of life of each species determines when its reproductive powers begin to decline, and the relation is, therefore, exactly the reverse of that which Weismann assumes; for the cessation of the reproductive power is determined by the decline of vitality and this decline is not regulated by the cessation of the power to reproduce.

5. That the process of selection of individual fluctuations can, at best, only keep the species up to an artificial standard that will be lost as soon as a rigorous process of selection ceases, has been shown by de Vries and others. The permanent inheritance of each species can not have been acquired in this way. Unless, for example, those individuals whose life is somewhat longer or shorter were being constantly destroyed *in every generation* the little would soon be lost that had been so laboriously gained. It is needless to point out that no such process is taking place on a scale sufficient to regulate the evolutionary process.

6. The length of life of a species is something that is as characteristic of the species as any of its structural or physiological properties. To state that the duration of life can not be supposed to be the result of physiological processes is not simply paradoxical but absurd. The paradox and also the absurdity disappear as soon as we recognize the fact that the length of life is a characteristic of each new species and is a purely physiological process. Those new elementary species whose physiological processes fulfill the conditions of a certain environment survive, those that do not perish; and there is no subsequent lengthening of one character and shortening of another, as on a sliding scale, to fit the new form in all details to its new environment. The length of life is predetermined with the advent of the new form, and is not subsequently regulated for the benefit of that particular species. From this point of view we get a simple and clear analysis of the problem, while that which Weismann maintains leads only to an unmeaning 'paradox.'

It seems to me that the method of the Darwinian school of looking upon each particular function, or structure, of the individual as capable of indefinite control through selection is fundamentally wrong. For instance, in regard to the number of eggs characteristic of each species, it is assumed that the output is also regulated by means of selection. On the contrary it appears to me that the power to produce a certain number of eggs is one of the fixed characteristics of each species that appears, and is not increased or diminished by external needs. The number of individuals that reach maturity will stand, therefore, as a measure of how far a new species is from the beginning adapted to the old environment, or to the new one in which it establishes itself. There may be a wide range of perfection in this respect, for there are some species that produce few eggs, but succeed in bringing a large number of them to maturity, and there are other species which, despite the countless number of eggs that they produce, only succeed in barely holding on to existence. Since the great majority of lower animals and plants produce large numbers of eggs we may infer that the arrangement for propagation, while it suffices to keep the species in

existence, is extremely wasteful, and far from being as perfect as we can easily imagine it might become if the process could be regulated by individual selection.

An Adaptation may be More Perfect than Survival requires.

Are organisms ever more perfectly adapted in certain characters to their environment than the demands of survival require? A positive answer to this question might release us in part from the modern test of utilitarianism.

It is a well-known fact that through use many, perhaps all, parts of the body are capable of doing more than they are called upon to do during the ordinary life of the individual. The muscles through practise not only become larger and stronger, but can even be educated to do more rapid work, as seen in the fingers of the skilled pianist. The sensation of touch can be made more perfect through practise. The skin thickens wherever continued pressure is brought to bear on it. The bones will change their form, and even make new sockets under suitable conditions. The walls of the blood vessels become thicker if more blood is thrown into the blood channels. These are typical examples of what the body is capable of doing, and the responses in each case are obviously to the advantage of the individual. What is the meaning of this power to do more than the ordinary requirements of life demand?

It has been suggested that the survival of the individual is sometimes determined by its capacity to rise to extreme occasions. For example, the deer that is capable of putting on a little more speed than its fellows, is the one that escapes. But this assumption fails to meet fully the case, for, in the first place, it assumes as already present a certain amount of the very quality to be explained. In the second place a similar capacity is also present in other organs, in which it is highly improbable that the power to improve somewhat could be of sufficient importance to be decisive in a life and death struggle. It could be of little advantage for instance to have the power of improving the musical sense beyond a very low average, and no one will suppose that this has been decisive in the evolution of the race.

In other directions also we find an apparently superfluous perfection of development. It is improbable that the extraordinary adjustments of which the eye is capable have all been acquired little by little through a life and death struggle. The eye is, however, such an important organ for the welfare of the individual, that it is hard to demonstrate positively that each stage was not of great use, but for the ear it seems improbable that its perfection in certain respects could have been of vital importance for the maintenance of the race.

The symmetry of animals and of plants is also in many cases

more perfect than the requirements demand. The almost exact resemblance between the right and left sides of the body, while advantageous up to a certain point, is often far more perfect than competition requires. In fact we find the Darwinians often dodging this consideration, and referring the results to 'the laws of growth,' etc. But if these 'laws of growth' exist why may they not have also carried the perfection of any organ far beyond the point at which the test of survival stops?

We meet with a somewhat similar case in the distribution of color over the bodies of animals. Granting that in some cases the presence of colors, after they have appeared, may be of some use to the animal or plant, yet the wonderful symmetry of distribution, and the gradual shading of the colors is often far more regular than appears to be required. This result also will no doubt be ascribed to 'the laws of growth,' but if we once admit such a principle at work, why bring in any other outside law to explain the perfection of those characters that are useful to the organism? If when a new species appears its colors happen to be so distributed that the individual gains some advantage, so much the better! If the color does not count one way or the other, then it does not enter into the problem of survival; but if it exposes an animal to a greater risk than it can surmount, that species will fail to hold its own. The same regularity and graduation of color exists also in animals that are microscopic, and no one thinks of accounting for these conditions through selection. Why then do we need a special explanation when the animals are so large that they attract our attention?

A number of writers, of whom Eimer is perhaps the most prominent, have insisted that evolution progresses along certain definite lines that are quite independent of selection of individual variations. The process has been called orthogenesis. Certain aspects of this view are in full accord with the theory of the survival of definite variations; for, we find, in fact, one of the most characteristic features in the appearance of definite variations to be that the same forms appear over and over again, showing a definite tendency to vary in certain definite directions. A striking case of this sort is that of the japanned peacocks described by Darwin, and of the mutations of the evening primrose described by de Vries. If future work can show that a change in a given direction is likely to be followed by others in the same direction, amongst some at least of the offspring, the process will have much in common with the process of orthogenesis. If this process should be in the direction of making some particular organ more perfect than the conditions demand, the new type may persist along with the parent form also, which it need not replace. If, on the contrary, the new acquirement unfits the new species for its environment the new species

will never become established. Cases of this sort, in which a species continually gives off mutations, that can not survive, and yet continue to appear, are known.

In most cases the survival of a species is not determined by one particular character, but by the sum total of all. Therefore since the characters mutate independently, we might expect to find occasionally in a new species some characters more perfect than the actual requirements demand; other characters less perfect than is necessary to maintain the species if survival depended on these alone, and many characters that suffice for the demands of survival. Thus in man, to take but a single example, the ear and the eye may be more perfectly developed in some respects than the demands of survival require, while the appendix vermiformis is actually injurious to the welfare of the race. The majority of the peculiarities lie somewhere between these extremes. A new race of men can not be produced by selection of those individuals that show fluctuating variations in these different directions, but must arise by the sudden appearance of a new type or types, which, finding a foothold, may establish themselves along side of the present races.* It may be that definite variations are even at present occurring, but are not of sufficient importance or difference to attract attention. Permanent improvement must be looked for in this direction, and not from the picking out of those individuals that fluctuate in an advantageous direction.

How Adaptation may arise through the Appearance of Definite Variations.

If we take the position that the creation of new forms is not the outcome of a long continued process of remodeling of each species, but that new forms appear 'spontaneously,' how can we account for the adaptation of new species to their environment? Are we to believe that all new forms that appear from 'inner causes' will be already adapted to some external set of conditions? A very slight familiarity with living things will give, I believe, a negative answer to this question. Nevertheless let us not conclude too quickly that none of the new forms will be adapted to some environment, even if some of them are not.

Darwin defined natural selection as follows: 'This preservation of favorable individual differences and variations, and the destruction of those which are injurious, I have called natural selection, or the sur-

*I do not, of course, mean to imply that any one of the present races of mankind could not be greatly improved artificially by encouraging the individuals best suited to civilized conditions to propagate, and by discouraging propagation by the criminal, indolent and unhealthy individuals. Until this is done we can not hope for even an artificial improvement in the standard of the race.

vival of the fittest.' He constantly compares the action of natural selection to that of artificial selection, in which he supposes that the breeder picks out those individual differences of the kind known as 'fluctuating variations.'

Modern zoologists who claim that the Darwinian theory is sufficiently broad to include the idea of the survival of definite variations seem inclined to forget that Darwin examined this possibility and rejected it. The grounds for this rejection seemed valid at the time, but a wider knowledge of the facts has shown that the problem is simpler than Darwin was aware of.

While Darwin uses the term 'struggle for existence' in a very loose, and often in a metaphorical sense, as he himself points out; and while it is true that he speaks of varieties and even species struggling with each other, yet the central idea is that natural selection adapts the organism to its environment by picking out and accumulating slight, individual differences. It is, indeed, only in this way that natural selection appears in the rôle of a creative factor in evolution, and it is this power to build up new adapted types that appears to give the theory its high value.

Numerous cases of discontinuous variation have been known for a long time, so that it is no mere assumption that such occur in nature. Darwin himself has collected many instances of this sort, and amongst domesticated animals and plants sudden variations have been frequently recorded. In fact there can be no doubt, especially in the case of plants, that such variations have often been utilized by the breeder, even unconsciously at times. A few cases of this sort have been described even for domesticated pigeons, and it is not improbable that the great variety of domesticated breeds may, in part, have arisen in this way, and not as the result of the selection of the individual fluctuations of the wild rock-pigeon. Darwin argued that sudden variations can not have been the source of new species, because, as a rule, when they are crossed with the parent form the offspring are not intermediate, but are exactly like one or the other parent, and it is a well known fact that when wild species are crossed the hybrid is midway in character between its parents. Therefore, Darwin believed, wild species could not have arisen as discontinuous variations.

Our knowledge on these points has greatly increased since Darwin's time, especially in recent years, mainly as the result of de Vries's work on the evening primrose. It appears that there are several different kinds of definite variations. The simplest cases are those in which some one character suddenly becomes lost, as when an albino mouse arises from a gray mouse. If such an albino is crossed with the parent gray form the offspring in the first generation are all like the gray mouse, but if these offspring are then inbred, they will produce some

pure white which breed pure white, and some pure gray mice which breed pure gray, besides some impure forms like those of the first generation, which breed both colors. In other cases the new form if bred to the parent stock gives rise to offspring which even in the first generation are like the new form. Thus it appears that when the ancon ram was bred to an ordinary ewe the offspring were like the father. Similar cases are recorded for the merino sheep, for the japanned peacock, and for other new types. It appears here that the new type dominates in some or in all of the offspring. Darwin had in mind cases like these when he rejected the view that discontinuous variations have furnished the material for natural selection.*

There are, however, still other kinds of sudden and definite variations, and these, not the former, are the kind which are mainly responsible for new species. New elementary species arise not through a loss of some character of the old form (although new species may have amongst their new characters some that are due to loss), but by the appearance of a new character or characters which follow a different law of inheritance. When such elementary species are crossed with the parent type, a new type may arise, which is usually intermediate in character between the parents, but in some cases may be more like one than the other. Similar results follow when the new elementary species are crossed with each other. Thus the results are comparable to those that occur when Linnæan species are crossed, although in the case when forms belonging to two widely different Linnæan species are combined the results may be so complicated that it is beyond our power to give a satisfactory analysis of the results at present.†

The term mutation has been used for the sudden appearance of new variations belonging to any one of the preceding cases. Whether it may not prove necessary to use different terms for the different kinds of discontinuous variations in so far as they follow different laws of inheritance remains for the future to decide. We might, perhaps, use the term *mutation* (and mutant) only where new elementary species are formed; *retrogression* where a character is lost (elementary varieties being formed, which follow the Mendelian law of inheritance, as

* It is not clear from the records whether this class follows the Mendelian rule of inheritance, with the new form as the dominant in the first generation. From the statements that are ordinarily made, it might appear that even in the first generation some of the offspring are like one parent and some like the other, which, if true, would make these cases different from the first class given that follow Mendel's law of inheritance.

† In my book on 'Evolution and Adaptation,' I pointed out that while a single step or mutation could not be halved, yet if a number of steps were taken in the same direction the number might then be halved. This may be true but is unnecessarily hypothetical, and, as pointed out here in the text, the apparent difficulty that Darwin met with can now be explained in other ways.

in the case of albinos, etc.); *atavism* when a latent character becomes active, as when a lost character reappears; and *saltation* where a new combination of characters is produced and where the new form being crossed with the parent-stock does not give the Mendelian ratio, as in the case of the ancon ram, the japanned peacock, etc., *if*, in fact, these cases do not really follow the same rule of inheritance as do the elementary varieties.

De Vries has pointed out that each step, each mutation, may not have been any greater than the difference between the extremes of fluctuating variations, and if this is the case we see that evolution may have been a very *gradual* process, although not necessarily a very *slow* one. Darwin's idea that the process of evolution was very gradual is in full harmony with the mutation theory, but on the latter view we can better understand how evolution may at times have been relatively rapid, and that no such enormous periods are required for the process, as the Darwinian school is inclined to assume.

The time has come, I think, when we are beginning to see the process of evolution in a new light. Nature makes new species outright. Amongst these new species there will be some that manage to find a place where they may continue to exist. How well they are suited to such places will be shown, in one respect, by the number of individuals that they can bring to maturity. Some of the new forms may be well adapted to certain localities, and will flourish there; others may eke out a precarious existence, because they do not find a place to which they are well suited, and can not better adapt themselves to the conditions under which they live; and there will be others that can find no place at all in which they can develop, and will not even be able to make a start. From this point of view the process of evolution appears in a more kindly light than when we imagine that success is only attained through the destruction of all rivals. The process appears not so much the result of the destruction of vast numbers of individuals, for the poorly adapted will not be able to make even a beginning. Evolution is not a war of all against all, but it is largely a creation of new types for the unoccupied, or poorly occupied places in nature.

Conclusions.

In the preceding pages I have tried to bring into contrast the point of view of the Darwinian school and the newer conception of the survival of elementary species. I have tried to show what selection has meant to the selectionist. They have never hesitated to take each particular character of an animal or plant, and dress it up in more perfect garments, while the body of the species, if I may so speak, has been left as it was before. There has been a continual tampering with the characters of the organism with the laudable intention of doing

with them that which nature herself seems unable to do, namely, to dissociate them from the rest of the organization and perfect them in this way or in that. It is this meddling with the fluctuating characters of the species that has been the characteristic procedure of the Darwinians, in their attempt to show how new species have been created. In contrast to this method, the theory of the survival of species assumes that a form once made does not have its individual parts later disassociated and adjusted to better fit the external needs of the species. Such a new form can change only by becoming again a new species with a new combination of characters; some of which may be more developed in one direction than before, others less; etc.

New forms on the Darwinian theory are supposed to be created by a process of picking out of individual differences. If, in addition to this, Darwin supposed that at times varieties and species crowd each other out nothing new is thereby created.* On the other hand the theory of the survival of definite variations refers the creation of new forms to another process, namely, to a sudden change in the character of the germ. The creating has already taken place before the question of the survival of the new form comes up. After the new form has appeared the question of its persistence will depend on whether it can get a foothold. The result is now the same as when species crowd each other out. This distinction appears to me to be not a matter of secondary interest, but one of fundamental importance, for it involves the whole question of the 'origin of species.' So far as a phrase may sum up the difference, it appears that new species are *born;* they are not *made* by Darwinian methods, and the theory of natural selection has

*If the survival of certain species determines, in a metaphorical sense, the kinds of future mutations that occur, the course of evolution may appear to be guided by selection or survival; but however true it may be that selection acts by lopping off certain branches, and limits to this entent the kinds of possible future mutations, the *origin* of the new forms remains still a different question from the question of the survival of certain species. This negative action of selection is not the process that most Darwinians have had in mind as the source of the origin of new species. It is true that Weismann believes that selection of individual differences determines the origin of new species, and that the creation of these new species determines the future course of variations in the same direction, but his argument that fluctuating variations can go on indefinitely varying in the direction of selection is refuted by what has been actually found to be the case when the process of selection of fluctuating variations is carried out. Most of the individuals of a species may be brought in this way to show the particular character selected in its highest degree as a fluctuating variation, but it appears not possible to transgress this limit; and rigorous selection in every generation is necessary to hold the individuals to the highest point reached. Only by the appearance of new definite variations can a given character be permanently fixed, or a new species created that will show fluctuating variations around the new standard.

nothing to do with the *origin* of species, but with the survival of already formed species. Not selection of the fittest individuals, but the survival of the *sufficiently* fit species.

There is a fundamental difference between the idea that fluctuating variations become specific characters through accumulation by selection, and the idea that new species arise as definite variations, which, with their appearance, characterize the new form as a new species. According to the Darwinian theory, natural selection performs a double duty, first, to build up new species, and, second, to maintain them in competition with other species. According to the other view, species are not formed by any kind of selection, and the question of survival only concerns the maintenance of species, already formed. The primary problem is the problem of the 'origin of species.' The central idea is not what species survive, but how species originate; no matter whether they are going to become victorious or not.

After a species has appeared it will surely be admitted by every one, that forms that can survive will survive! If Darwin's theory meant only this to those who adopted it, is it not surprising that such a truism should have been hailed as a great discovery? Was not the theory heralded because it seemed to explain how new species arose? What shall we say then when we find a situation like that existing at the present time, when we are told that after all the only difference between Darwin's theory of natural selection and the theory of the survival of definite variations is that in the one case fluctuating variations are selected, and in the other mutations, and that in both cases natural selection is the key to the evolutionary process! Is not the 'origin of species' still the real point at issue?

I yield to no one in admiration for what Darwin has done in behalf of the biological sciences, for he succeeded, where the great French zoologists failed, in establishing the principle of evolution. Furthermore no other hypothesis, that has as yet been proposed, accounts so well for the widespread *occurrence* of adaptation of organisms to the environment as does the principle of natural selection. But appreciation of Darwin's claims in these directions need not blind us to the insufficiency of the theory of natural selection to account for the *origin of species;* nor to the fact that his followers have been especially concerned in propounding and making application of this side of the theory. They have shown little interest in selection as the great conserving factor of evolution, and the reason for this is not far to seek, because of the much greater importance that they have attributed to natural selection as a creative factor in building up individual differences into specific characters.

GALILEO.

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III.*

AN extant annotation dated February 26, 1616, which is undoubtedly genuine, declares that upon this day Galileo was summoned before Cardinal Bellarmine and in the presence of witnesses was warned of the error of the Copernican opinion taught by him, and was admonished henceforth not to hold, teach or defend it in any way whatsoever, verbally or in writing . . . which injunction the said Galileo promised to obey. The exact wording should be noticed. Upon it the subsequent fate of Galileo hangs. The document is genuine. Does it represent the facts of his examination of 1616 exactly as they occurred?

The proceedings against Galileo in 1632–3 show that the Pope and the Holy Office acted precisely as if the statements of the annotation were exact. The publication of his *Dialogues* (1631) was a *flagrant* violation of the command not to teach, etc. In the case of a personage so celebrated as Galileo nothing less than a flagrant violation would be noticed. The Roman Curia could not afford to harass him about trifles. With his defense of 1633 he submitted the following certificate:

> We, Roberto Cardinal Bellarmine, having heard that it is calumniously reported that Signor Galileo Galilei has in our hand abjured and has also been punished with salutary penance, and being requested to state the truth as to this, declare: that the said Signor Galileo has not abjured . . . any opinion or doctrine held by him,† neither has any salutary penance been imposed upon him; but only the declaration made by the Holy Father and published by the Sacred Congregation of the Index has been intimated to him, wherein it is set forth that the doctrine attributed to Copernicus, . . . is contrary to the Holy Scriptures and therefore can not be defended or held.‡ In witness whereof we have written and subscribed these presents with our hand this 26th day of May, 1616. Roberto Card. Bellarmino.

Galileo's enemies had spread the calumnious reports mentioned. He wished to have a proof that they were false. Cardinal Bellarmine

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* Continued from the POPULAR SCIENCE MONTHLY for February, 1905.

† According to the terms of this certificate Galileo never had 'held' the Copernican opinion—or at least he had never 'abjured' it. The annotation of February 26, 1616, commands him to 'relinquish' it.

‡ The words respecting *teaching* are not here given, it is to be remarked.

was his friend and admirer and at his request gave this certificate. Bellarmine died in 1621 and could not be called as a witness in 1632. When Galileo was called upon to defend himself for teaching the Copernican doctrine in his *Dialogues,* which had given great offense, he produced this certificate and called attention to its wording, which differs materially from that of the protocol of February 26, being much less stringent in form. In essence it is the same; to teach a doctrine as true is to 'defend' it. Cardinal Bellarmine did not have the protocol before him in writing the informal certificate. The prohibition of the latter is, however, precise and absolute; the doctrine 'can not be defended,' that is, taught in any way as if it were true. It can not even be 'held,' silently. It represented the attitude of the cardinal's mind precisely; the church would not suffer if its terms were obeyed. In reading Galileo's defense of 1632–3, we shall see the use he made of the discrepancy between these two documents, one formal and of record (February 26), the other friendly and informal (May 26).

It is the theory of Gebler in his careful history, 'Galileo Galilei and the Roman Curia,' that the genuine document of February 26 is not a true record of the facts. He admits that it was written in its proper place by the notary. He finds an 'obvious contradiction' between a formal command 'not in any way to hold or defend,' which are the words of the process of 1633, and the prohibition of Bellarmine's certificate 'not to defend or hold.' After an examination of all the documents it is impossible, I think, to take Gebler's view. It is necessary to admit the words of the genuine documents to mean precisely what they say.

Gebler lays down three facts as indisputable: '(I.) Galileo did not receive any prohibition except the cardinal's admonition not to defend or hold the Copernican doctrine; (II.) Entire silence on the subject was therefore not enjoined upon him; (III.) The second part of the note in the Vatican MS. of February 26, 1616, is therefore untrue.' My own conclusions are entirely different as to all three prohibitions. The Cardinal's admonitions are, in effect, absolutely the same as those of the formal prohibition; silence was enjoined, and more than this Galileo was forbidden to hold certain opinions even mentally and silently. If not, what does Bellarmine mean by the word 'hold'? Is it, I ask, credible that an authority that forbids a man to hold an opinion, even silently, would permit him to teach it? To ask the question is to answer it. When Galileo taught the opinion he disobeyed the orders of a Church whose authority he fully admitted during the whole of his life.

Within the assigned limits of this paper the matter can not be discussed at length. Two points may be touched upon however.

Galileo's letters to Florence in 1616 do not mention the prohibition to himself for two good reasons, *first,* to divulge the proceedings of the Holy Office would have been a serious matter; *second,* Galileo had every reason for convincing his friends that the Holy Office had only come to decisions 'purely public' regarding the Copernican doctrine, and 'not affecting my [his] personal interests' (letter of March 6, 1616).

Again, the protocol of February 25 gives the orders of the Pope that certain things should be done 'in case of his refusal to obey.' It does not explicitly enjoin or prohibit the same action after his promise of obedience. Cardinal Bellarmine had full power in such a matter. If Galileo had refused to obey he would have been imprisoned. When he had promised to abandon the opinion of Copernicus the obvious step for Cardinal Bellarmine was to bind him to effective silence by a formal promise before witnesses. The protocol of February 26 recites that this was done. The words mean, I am obliged to conclude, precisely what they say. It must not be forgotten that Galileo, like every other good Catholic, had been forbidden to hold the Copernican opinion by the general prohibition of March 5, 1616.

The reigning Pope was Paul V., who hated 'science and polite scholars.' He was very civil to Galileo, however, received him graciously (March 11, 1616) and promised him safety from his enemies. Galileo was a celebrity; by his submission to authority he had averted a great scandal in the church; accordingly the Pope was gracious. For the next seven years (1616–23) Galileo's conduct precisely agrees with the supposition that he recognized that he must not teach the Copernican doctrines. He published nothing during this period. The authorities at Rome were engaged in 'correcting' the work of Copernicus. Galileo eagerly waited for the corrections, for they would be authoritative and would exhibit the limits within which it would be permitted to 'teach.'

In May, 1618, he sent a MS. copy of his treatise on the tides to Archduke Leopold of Austria, who was friendly to him. It implicitly assumes the truth of the Copernican doctrine "which I then (1616) held to be true* until it pleased those gentlemen to prohibit the work and to declare that opinion (of Copernicus) to be false and contrary to Scripture. Now, knowing as I do, that it behoves us to obey the decisions of the authorities, and to believe them, since they are guided by a higher insight than any to which my humble mind can attain, I consider this treatise which I send you merely to be a poetical conceit, or a dream, and desire that your Highness may take it as such . . . ." The words are ironical. They will have less effect

* In this letter Galileo declares that he 'held' the opinion till 1616. He then 'relinquished' it (February 26), but never 'abjured' it (May 26).

upon us when we remember that the science of this treatise of Galileo's is quite erroneous. It denies that the moon controls the tides. The treatise was not published. It was shown in MS. to a few trusted friends, but the ideas here set forth were developed in Galileo's *Dialogues* published in 1632.

In 1618 three comets appeared in the sky. Galileo communicated his views of their nature to a few friends. He considered them to be merely atmospheric appearances which rise far beyond the moon, to be sure, and not heavenly bodies. The conclusion was erroneous, of course. In 1619 the Jesuit Father Grassi delivered a lecture in Rome maintaining that the comets were heavenly bodies (as they are). Galileo induced one of his pupils to reply to Grassi, and himself corrected the MS. work so that its severe criticisms of the Jesuit (who was, after all, defending a true thesis) are Galileo's own. A reply was written by Grassi in which Galileo is personally attacked and the Copernican system assailed. Galileo's answer is the famous *Il Saggiatore* (the assayer) which was printed October, 1623. It was brilliantly, but very carefully written, and before it was published it passed from hand to hand among Galileo's friends, who purged it of every phrase likely to be dangerous. The *imprimatur* was given on a report of Father Riccardi,* a former pupil of Galileo's, of whom we shall hear more.

In July Pope Gregory XV. died and was succeeded by Urban VIII. who, as Cardinal Maffeo Barberini, had for many years been one of Galileo's strongest supporters. A new era seemed to open with his accession. His many letters to Galileo had always been friendly, often cordial. In thanking Galileo for his letters on solar spots (1613) the cardinal had written: "I shall not fail to read them with pleasure, again and again, which they deserve. . . . I thank you very much for your remembrance of me, and beg you not to forget the high opinion that I entertain for a mind so extraordinarily gifted as yours." In 1620 the cardinal composed a poem in Galileo's honor and sent it to him as a 'proof of great affection.'

During the progress of Galileo's affair with the Holy Office in 1615 and 1616, the cardinal stood his friend and believed that it was chiefly to his own efforts that an issue so satisfactory to the astronomer personally was brought about. He was a friend to Galileo; he was not a believer in the Copernican doctrine; he made no efforts to prevent its condemnation. He proved to be inexorable where the interests of the papacy were, or seemed to be, involved. His accession was hailed by Galileo's friends, and *Il Saggiatore* was dedicated to him, and he accepted the dedication. The book is considered a model of

* Riccardi had been convinced that the Ptolemaic theory was false and had accepted in its place not the theory of Copernicus, but that of Tycho Brahe.

dialectic skill and a literary masterpiece. The original controversy about the comets is almost lost sight of. The errors of Grassi are shown up mercilessly. The Copernican system, which Galileo 'as a pious Catholic considers entirely erroneous and completely denies' is covertly defended. It is shown to agree with the revelations of the telescope; and these are proved to be inexplicable on any other system. As the Copernican opinion is, however, condemned by the church, as Ptolemy's is untenable, and Tycho's inadequate, Galileo concludes that some other system must be sought for.

In this brilliant essay—which was withheld until Galileo's powerful friend was seated in the pontifical chair—Galileo held, taught and defended the Copernican doctrine. It was supposed to be, at least, safe for him to do so in a covert way. The book was read by the Pope, who enjoyed it highly—so Galileo heard. It was examined by the Inquisition and no action was taken. By these and other signs Galileo judged that an attempt to remove the condemnation of the Copernican system might now, at least, succeed. Its weightiest opponent, Cardinal Bellarmine, an earnest, sincere and learned man, had died in 1621. Galileo proposed to go to Rome to congratulate the new Pope on his accession. The proposal was well received. Friends wrote to him: "I swear to you that nothing pleased his holiness so much as the mention of your name . . . the Pope replied that it would give him great pleasure, . . . if the journey would not be injurious to your health; for great men like you must spare themselves that they may live as long as possible."

Galileo arrived in Rome towards the end of April, 1624. He was received with the greatest honor. Every one knew the Pope to be his friend and that he had many supporters among notabilities. In the space of six weeks he was granted six long audiences with the pontiff. The Copernican system was discussed. Galileo argued warmly in its favor. He met with no success, while the Pope replied with arguments of his own against it. The new doctrine was not to be tolerated. Certain of the cardinals, at Galileo's request, engaged in the matter. The Pope was inexorable. No one can decide now what the Pope's arguments were. From the whole course of events, it seems probable that he was not satisfied that the Copernican theory was true; and it is evident that his mind was made up to allow no scandal to arise from its teaching. Galileo returned home loaded with favors. A pension was promised to his son. The Pope gave him a splendid picture, and two medals, and furthermore addressed a letter to the Grand Duke of Tuscany (June 7, 1624) in which he declares that Galileo's great discoveries 'will shine on earth so long as Jupiter and his satellites shine in heaven.' 'That you may fully understand to what extent he is dear to us, we wish to give this brilliant testimony

to his virtues and piety.' 'We have observed in him not only literary distinction, but also the love of religion and all the good qualities worthy of the papal favor.'

Galileo was again at the very summit of prosperity. He thought it safe, on his return to Florence, to write a reply to an Italian advocate, Ingoli, in which he defends the Copernican theory. In the first place he shows that he formerly defended it because of its inherent probability. He proves that he had not defended an idea improbable or unreasonable in itself. Again he desires to show the Protestant Copernicans in Germany that the heliocentric doctrine had not been rejected in Italy from ignorance of its great probability, but from reverence for Holy Scripture, zeal for religion and our holy faith.

Il Saggiatore had been well received. Why might he not go further under the favor of the Pope? All reports from Rome were favorable. And indeed he had heard (December, 1625) that the Pope had listened to several passages from this last pamphlet and had highly approved them. If he had gone so far, why then might he not go still farther? On the surface of affairs there was no apparent reason. Up to this time Galileo had preserved the forms fully. He professed not to hold Copernican doctrines. Not holding them, how could his writings be taken as teaching or defending them? The Pope, his friend, had not disapproved his previous writings. Galileo misinterpreted this as a sign of his toleration of the doctrines. It is now apparent that the Pope's whole course was consistent. He desired to give Galileo every liberty, but was sternly set against any teachings that would diminish the authority of the Church. From first to last he was unconvinced of the scientific truth of the Copernican opinion. He had personally befriended and honored Galileo. He looked for a grateful acknowledgment in return. Galileo had been denounced by his enemies, but they were overawed, and would certainly take up no quarrel in which he was not flagrantly disobedient to the prohibition of 1616. *Il Saggiatore* had been a brilliant success. He now set about arranging another work—the *Dialogues on the two principal systems of the World*—parts of which had been in hand for some years.

This is the place to record Galileo's share in the invention of the microscope. While he was in Rome (1624) a complicated microscope was shown to him that had been invented by Drebbel, a Dutchman. Galileo simplified and greatly improved it. His relation to the invention of the telescope and of the microscope is the same. The first ideas came from others; Galileo put them into practical forms. The real inventor of the microscope is not Drebbel, but Zacharias Jansen, a spectacle maker of Middleburg who made the first instruments in the last years of the sixteenth century, before the telescope was invented, therefore.

Galileo's dialogues on the system of the world (1632) have, at the head, a Greek epigraph:

In Every Judgment beware of Your Prejudices!

They are dedicated to the Grand Duke of Tuscany. The personages of the *Dialogues* are Salviati (Galileo himself) who maintains the Copernican doctrines; Sangredo, a man-of-the-world, intelligent, but not a savant; and Simplicius, a convinced Aristotelian, a dull fellow, always worsted in the argument. Galileo's enemies convinced the Pope that Simplicius stood for the Pontiff himself. The subjects discussed are the fall of bodies, the flight of projectiles, the principles of mechanics, the rotation and revolution of the earth and of the planets, the system of Ptolemy—and here Sangredo remarks that he knows many disciples of Ptolemy who have become Copernicans, but not one Copernican converted to the ancient system. The new star of 1572 is shown to have been far more distant than the moon, by long calculations (and it is noteworthy that logarithms are not employed to shorten the work). The preface recites that, some years previously a 'salutary edict' had been promulgated at Rome which, to prevent scandals, forbade the teaching of the Pythagorean opinion of the earth's motion,* that some hardy spirits had, nevertheless, dared to declare that this edict had been issued without comprehension of the matter and that it was the result of passion, and not of judicial examination. It had been said that advisers entirely ignorant of astronomy ought not to have thus clipped the wings of philosophers.

My zeal, says Galileo, can not support these rash complaints. Well understanding this prudent decree, I wish to do justice to the truth. I was then at Rome; the most distinguished prelates heard and applauded me; the decree would not have been issued without giving me some knowledge of it. I, therefore, wish to show to foreign nations that in Italy, and even at Rome, all that could be advanced in favor of Copernicus was known, before that censure was published. I declared myself the advocate of Copernicus. Proceeding according to a mathematical hypothesis, I endeavored to prove it to be preferable to that which declares the earth at rest, not in an absolute manner preferable, but in the sense in which it is attacked by pretended Aristotelians, who in their philosophizing neglect observations. He will show, he says, certain advantages of the heliocentric system. If Italians have not assented to the mathematical opinion of the motion of the earth, it is not because all of them have been ignorant of the reasons others allege in its support, but because they have other reasons

* This general edict, of course, included Galileo, whether there was any special command laid upon him or no.

based on piety, religion, on a knowledge of divine omnipotence and the weakness of the human understanding. It is the opinion of good authorities that the foregoing introduction was first written by Galileo, then revised by the censor at Rome—perhaps by the Pope himself—and finally returned to the author with permission to make such verbal changes as would not alter the sense of the Roman revision.

In the *Dialogues* the three interlocutors proceed to construct a scheme of the universe, step by step. The construction is made by Simplicius, and the system proposed by Copernicus and demonstrated by Galileo emerges triumphant. All the glory is for Copernicus and his advocate, Galileo. No credit is assigned to Kepler for his discoveries which had done away with the whole apparatus of epicycles retained by Copernicus. Kepler is not mentioned here or elsewhere with praise. Simplicius objects to some mathematical reasoning because Aristotle recommended his disciples to abstain from geometry. Salviati thinks Aristotle wise; for geometry is the art by which his errors and deceits are discovered. As to the empty spaces beyond Saturn: who are we to judge of the greatness of the universe? Can we say that these spaces are useless because we see no planet there? May they not be peopled with invisible planets? Who suspected the existence of the moons of Jupiter? Who tells us that all the heavenly bodies were created for *us?* Certain authors—Kepler, for one—assert that tides are caused by the moon. Galileo will not waste his time in refuting such assertions. Nothing is so astonishing to Galileo as that Kepler, a free and penetrating spirit, should have assented to such 'ineptitudes.' Simplicius on his part declares that the tides are miracles. In all the book there is no discussion of Scriptural texts.

It is not necessary to carry the analysis of these famous dialogues further. The arguments employed are so familiar to us that we forget they were once fresh and novel. They were accepted by Galileo's contemporaries as witty and brilliant, and even now Italians admire their style, though most English readers find them, as a whole, prolix, not to say dull. The Copernican doctrine is enforced in every possible way. Every argument for the Aristotelian theory is brought forward, in turn, by Simplicius only to be utterly refuted. Sarcasm is unsparingly employed. Simplicius is not only wrong, but ludicrously so. After each unusually convincing passage Salviati is careful to add that, after all, the Copernican doctrine is a 'fantasy' or a 'vain chimera.' At the termination of the dialogues, which extend over four days, no general summing-up is made. The reader is left to draw his own conclusions. Salviati apologizes to Simplicius for the ardor of his language and assures him that he had no intention to offend him, but wished rather to stimulate him to communicate his 'sublime' ideas—ideas which have been utterly refuted in the course

of the book. 'Your reasons,' says Simplicius, 'are most ingenious; but I do not believe them to be either true or conclusive.' Then Simplicius recalls a wise reflection, made formerly, in his presence, by an eminent personage before whom all must bow, as follows: 'We observe,' he says, 'nothing but appearances; by what right do you presume to limit the power of God by fixing the ways in which it has pleased Him to produce them?' These are the very words spoken by Pope Urban to Galileo in 1624. They were considered conclusive by the Pope. In the mouth of Simplicius they ring hollow.

It must not be forgotten that Galileo's theory of the tides upon which the *Dialogues* turn is, in itself, entirely erroneous. The tides are not due to the moon, he says, but to certain motions of the earth, which are then discussed. The first motion is its rotation round an axis, the second its motion of revolution about the sun, and there is a third motion by virtue of which its axis of rotation is constrained to pass always through the same stars. The third motion (invented by Copernicus) is superfluous. The axis of the earth is always parallel to itself as it moves round the sun. Two motions are sufficient to account for all the phenomena; the third does not exist. It was, however, upon this third motion that Galileo founded his theory of the tides, which is, therefore, baseless. Many of his arguments for the Copernican doctrine are irresistible. Those founded on the tides are, necessarily, erroneous.

To obtain the authority to print the *Dialogues* Galileo went to Rome (May, 1630), where his friend and former pupil, Father Riccardi, was censor (master of the Sacred Palace). Without the *imprimatur* nothing could be printed. When the *imprimatur* of the censor was once given to any book its author was *prima facie* relieved from responsibility. In the subsequent proceedings against Galileo it was charged that he obtained the *imprimatur* by a 'ruse.' The history, as understood at Rome, was briefly as follows: In May, 1630, Galileo took the MS. to Rome, submitted it to the master of the Sacred Palace (Riccardi) and asked permission to print. Riccardi wished, for greater security, to review the book himself. To save time, it was agreed that the book should be printed at once and that the sheets, leaf by leaf, should be sent to Riccardi. To carry out this plan the *imprimatur* was given for Rome. Galileo soon went to Florence and from thence asked the censor for permission to print at Florence. This permission was refused. Riccardi insisted that the sheets should be submitted to him according to the original agreement. The plague was then raging throughout Italy and it was impossible to transmit parcels from Florence to Rome on account of the quarantine.

It was finally arranged through the Tuscan ambassador, Niccolini, that the printing should be done at Florence under the condition of

the submission of the whole work to a competent theologian of the Benedictine order, and that the introduction and conclusion should be sent, before issue, to the censor at Rome. The whole matter was then transferred to the inquisitor at Florence and the book was printed with the entire approval of Father Stephani, who had been charged with its supervision. The introduction and conclusion were duly sent to Rome, but the Roman censor kept them for months without giving his approval or, in fact, without communicating with Galileo. It was clear that Riccardi was doubtful. Through the Tuscan ambassador at Rome renewed efforts were made by Galileo to obtain Riccardi's approval, and, in the meantime, without waiting for it (March 1631) the printing was proceeded with at Florence. Riccardi (April 28, 1631) at last answered Galileo's request, refusing the *imprimatur* until new conditions had been fulfilled. The censor, in this letter, recalled the fact that this original *imprimatur* was only given conditionally.

"Father Stephani," says the censor, "has no doubt subjected the book to a conscientious revision; but as he was not acquainted with the Pope's views he had no power to give any approval, etc." A desire to delay the whole matter is evident. Riccardi fears for himself; he knows the Pope's views; he is a firm friend of Galileo's also. After further negotiations (May, 1631) the whole matter was referred to the inquisitor at Florence with full powers. Riccardi conveyed to the inquisitor the 'views' that must govern his decision: The Copernican system must be treated only as a mathematical hypothesis; there must be no reference to Scripture; the introduction and the conclusion of the book the censor will send from Rome. Accordingly, they were sent with permission to Galileo to change the rhetorical style but not the matter. It is the opinion of certain good authorities that the Pope himself revised the introduction. The book was finally printed (February, 1632) with the *imprimatur* of Rome and also of Florence. The authorities at Rome had not seen the text of the *Dialogues*. It appears that throughout the long and vexatious delays Galileo obeyed all explicit instructions given by the censors. There were good reasons for removing the printing of the work to Florence. It is, however, certain that it would never have been authorized in Rome in its final shape.

(*To be continued.*)

THE CULTIVATION OF TOBACCO IN THE PHILIPPINES.

By A. M. SANCHEZ.
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TOBACCO was introduced into the Philippines soon after the Spaniards took possession, seed being brought from Mexico by Spanish missionaries. For many years little or no effort was made to restrict or encourage its cultivation, until 1781, when the cultivation and sale of tobacco was declared a state monopoly. In the tobacco-growing districts of Luzon, each family was compelled to grow a certain number of plants and deliver the entire product to the agents of the government; no tobacco could be reserved for the use of the planter. Houses were searched for concealed tobacco and fines imposed for infractions of the law. These harsh provisions occasioned many riots and disturbances, and the monopoly was finally abolished in 1882. Since then, the cultivation and manufacture of tobacco have been in the hands of private individuals and companies.

At present tobacco-growing is one of the principal agricultural industries in the Philippines. The greater part of the tobacco is grown in the Island of Luzon, principally in the provinces of Isabela, Cagayan, Union, Ilocos Norte, Ilocos Sur and Batangas. Small quantities of tobacco are grown in the Visayan and southern islands, the greater quantities probably being produced in Masbate, Tablas, Panay, Bohol, Leyte, Siquijor, Negros and Mindanao.

The best quality of tobacco is grown on the light alluvial soils of the Cagayan River, in the provinces of Isabela and Cagayan. Isabela tobacco burns smoothly and freely, with a pleasant taste. The leaves are smooth, small in vein, thin in texture, stretch and cover well, and have an agreeable aroma.

Philippine tobacco is highly esteemed in the Orient. Its agreeable aroma and flavor have won for it a high place among cigar tobaccos. When we consider the desirable qualities of Philippine tobacco, with the primitive methods of cultivation in use, the imperfect curing and fermentation it receives, and the modern methods of treating the crop in America and other countries, it becomes quite clear that with modern scientific treatment, Philippine tobacco would be greatly improved, if not raised to rank among the best tobaccos of the world.

Soils and Fertilizers.

In the Cagayan Valley, where the best tobacco is grown, the general character of the soil is a sandy loam, three feet or more in depth,

easily cultivated and in good physical condition. Small areas of loam or heavier soil are found in several places; but these are usually low depressions, where flood waters accumulate and stand long, thus allowing the fine particles of clay carried in suspension to settle on the surface of the ground. The lands cultivated to tobacco are the bottoms or lands subject to floods from the high waters of the Cagayan River. Once or twice during the rainy season these lowlands are flooded to the depth of several feet by water carrying in suspension considerable quantities of fine sediment rich in fertilizing matter which, on standing, is deposited on the surface of the soil. No artificial fertilizer is used on the tobacco lands, and plants are entirely dependent on the fertilizing action of floods to supply the necessary amounts of the elements of fertility.

While the amount of fertilizing matter annually furnished these soils by the overflows of the Cagayan River is considerable, there is no doubt that with the aid of fertilizers much better results would be obtained.

Growing Tobacco under Shade.

In the province of Isabela one hectare of land was devoted this year (1903–1904) by one of the tobacco companies to an experiment in growing tobacco under shade. The construction of the shade was the same in all essential particulars as that commonly used in the Connecticut Valley and in Florida for growing fine-grade Sumatra tobacco, except that bamboo and bejuco (rattan) were substituted for hard-wood posts and nails. Owing to unavoidable delays, the field was planted too late in the season to obtain a representative result this year. It was observed, however, that the plants under shade were not attacked by insects and had broader, finer and darker-colored leaves than the plants outside. The superintendent in charge of the work expressed himself as convinced of the practicability of this method for producing a fine grade of tobacco for wrapping purposes. The Insular Bureau of Agriculture will conduct an experiment the coming season in growing Sumatra tobacco under shade.

Methods of Cultivation in Use.

The first operation in growing tobacco is the preparation of the seed-bed. This the native usually makes near his house, or preferably, in some cleared piece of ground in the woods near by. New or virgin soil is preferred, so that the young plants can have a vigorous growth and be in suitable condition for transplanting. Any brush and stumps that may be on the land are gathered and burned until entirely reduced to ashes. The ground is then stirred with the native plow or with an iron bar having a flattened end with a sharp edge.

After breaking up all large clods and leveling the land, the seed is scattered broadcast on the surface and by means of planks pressed into the soil. Usually a shade is placed over the bed to protect the young plants from the direct rays of the sun. Forty to sixty days after planting, the plants are ready for transplanting. This begins about the last of December and is continued until the middle of February. Experience has shown that transplanting later than March does not give satisfactory results. The preparation of the field to which the young plants are to be transferred consists of plowing and harrowing, in which operations the corresponding native implements are used.

The ground being stirred, the native provides himself with a string of the same length as the width of the field. Each end of the string is securely attached to a stick for the purpose of laying out the rows. Sometimes the string itself is divided into spaces of two and one half feet by securely tying red ribbons at these intervals. These show where the plants are to go in the rows. After stretching the string across the field, the holes are made with a stick or bolo, about three inches deep and three inches wide. The holes are watered immediately after the plants are put in. The planting is done in the evening or very early in the morning, when the rays of the sun are not strong. After six or seven weeks from the transplanting the plants are usually ready for topping.

Topping.—When the flower buds begin to appear topping is begun. Usually from fifteen to twenty leaves are left on each plant.

Worming.—The battle with the worms begins in the seed-bed and does not end until the crop is harvested. No poisons are used, but the worms are caught by hand and killed. This is usually done in the mornings or late in the evenings.

Harvesting and Curing.—As a general rule the tobacco is primed; that is, the leaves of the plant are removed as they mature. It is customary to make five gatherings at intervals of about a week. The native collects the leaves and places them in large baskets which, when filled, are carried to the drying-shed in rough sleds or carts. If the soil is rich, a second profitable crop is produced from the suckers. As soon as the original crop is topped, suckers will sprout from each leaf. These, of course, are broken off as soon as they appear, otherwise they would hinder the growth of the leaves. When all the leaves have been primed from the original stalk, except three or four at the top, two suckers are allowed to grow from the bottom of the stalk. When the remaining leaves are removed, the stalk is cut just above where the suckers sprout and the field is immediately cultivated. It is claimed that when the season is favorable very good filler-tobacco is produced from the suckers.

In the drying-shed the leaves are sorted, usually into five classes,

according to the size, color and condition of the leaf. The sorted leaves are put on sticks about one meter in length, the classes being kept separately. One hundred tobacco leaves are put on every stick and this is hung on the drying-shed to dry. The drying-shed most generally found in this country is made in the shape of the native house with roof, but with uncovered sides. The sticks with the tobacco leaves are hung on supports, usually set at the following distances: The first about five feet from the ground, the next two feet higher, while the succeeding ones usually have a distance between them of from three to four feet. It is seldom that the native takes the trouble to cover the sides of his shed and thus protect his tobacco from the intense sunlight, strong winds and rains. I have seen large quantities of tobacco hung on rafters to dry in the open fields, without any covering whatever to protect the leaves from the searching rays of the sun, and from the rains and dews. Again, the practise is very common of hanging the tobacco on poles horizontally suspended from the floors of the native shacks. More attention should be given to the proper construction of drying-sheds if the best results from the curing are to be obtained.

When dried, the tobacco is taken to some company's curing-house, where the first fermentation is done. The tobacco during this fermentation is still on the sticks used for hanging. The piles are kept at a temperature of between 35° and 40° C., the object being to give the tobacco a little color for guidance in classification and in further curing. After the first fermentation is complete, the tobacco is divided into five classes according to length and quality of the leaves. In the second fermentation, which is made with the tobacco in fardos, the temperature is kept at about 55° C. The piles are frequently turned over to secure the proper heat and regulate the fermentation. The success of the operation depends principally on the experience and skill of the manipulator, as there are so many vital points that enter into consideration.

The curing-houses owned by the several tobacco companies are not constructed so as to get the best results from the fermentation. Usually ventilation is lacking, and it is very difficult to control the fermentation the way that it should be done in order to obtain the shades of color in the tobacco which the market demands.

Yields of Tobacco.

The yields vary greatly with the seasons and also with the character of the soil. An average for several years from one acre is 610 lbs. In favorable seasons 3,000 lbs. of dry tobacco have been obtained from one acre of land.

LIFE IN A SEASIDE SUMMER SCHOOL.

By Professor CHARLES E. BESSEY,
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LATE one night in August I boarded the staunch little steamer 'Queen City,' at Victoria, and steamed out upon the Strait of Juan de Fuca, and up the western coast of Vancouver Island. This remarkable body of water, fifteen miles wide and seventy miles long, is the common gateway for British and American vessels. The treaty which settled the boundary dispute many years ago (1846) fixed the international line in the middle of the strait, so that each country has a broad and deep water passage to the Pacific Ocean. As we proceed northwesterly up the coast on the British side of the strait, the rugged and rock-bound coast of Vancouver Island rises on our right, while across the water is the Olympic coast of Washington. Of both shores little more is known than the mere coast line and a narrow strip near the water. Back of the shore line are foothills running back to the mountains still beyond them and covered all the way with dense and almost impenetrable forests of cedar (*Thuya plicata*) and fir (*Abies amabilis*). We steam along slowly, for this is a dangerous coast, and there is a heavy swell on the water and enough fog in the air to obscure the details of the shore line.

It is broad daylight when we turn into the deep harbor of Port Renfrew, almost directly opposite Cape Flattery, and come up to the long wharf. Here we find Jackson, the genial little Englishman, who fills the several offices of harbormaster, postmaster, storekeeper and hotelkeeper. When the boat comes in from Victoria, as it does once in a week or ten days, Jackson is a very busy man, but then he has a long time in which to recuperate before the next arrival of the boat. While he is looking after the freight and luggage, and sorting over the mail, we go to the big summer hotel and ask 'Jim,' the Chinaman, to get an early breakfast for two. My companion is a genial geologist, who has been here before, and knows Jim, and how to persuade him into complying without too great delay. While waiting for breakfast we look northward over the harbor to the foothills which surround it, and whose sides are covered with dense forests down to the water's edge. I have rarely looked upon a scene of such natural beauty, and stood long feasting my eyes upon sky and mountain, and forest and water.

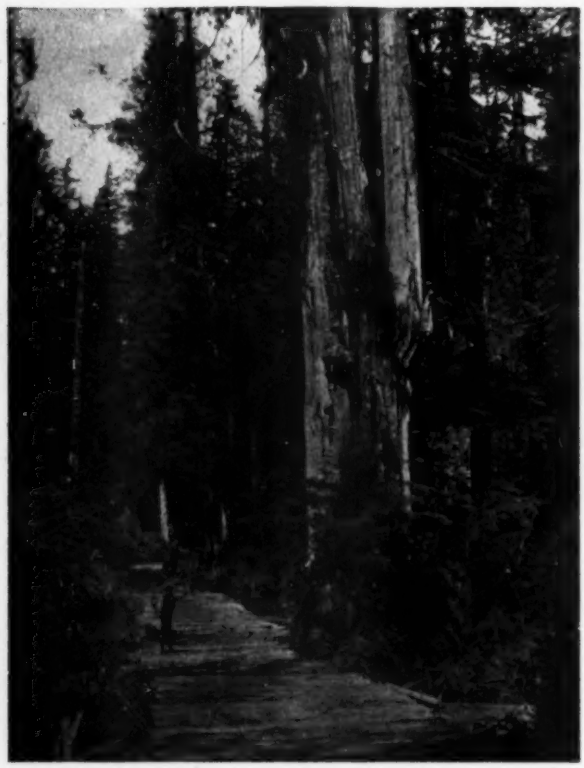
Breakfast eaten, and the camp mail secured, my genial geological friend advises me to prepare to take the trail to the camp several miles away, which I do by putting on canvas leggings and stout hob-nailed shoes. We try to dicker with the Indians (familiarly known as the 'Siwashes') to take our heavy luggage to the camp by canoe, but they are lazy and rapacious, and refuse to do so unless we pay several times the usual price, which we in turn refuse to do. So we take what we need most and start out over the trail. And such a trail! It begins fair enough, looking quite like an ordinary trail, but soon it changes into a mere path, and then abruptly drops down the slippery sides of a canyon, crosses a stream, and runs straight up

THE MINNESOTA SEASIDE STATION; LIVING HOUSE IN THE BACKGROUND; LABORATORIES IN THE FOREGROUND.

the other side. The geologist leads the way, carefully planting his feet in the notches in the canyon side, and I follow, thankful for the big hob-nails in my shoes. He jumps the stream, and so do I, and then he scrambles up the steep wall on the other side, and I follow, puffing and panting. At the next canyon the trail literally takes to the trees, crossing by a fallen tree whose trunk is slippery with damp mosses and lichens. Those blessed hob-nailed shoes do their duty, and I reach the other side safely, only to find my companion far ahead crawling under some fallen trees under which the trail runs.

But all things come to an end, and so does this wonderful trail.

LOOKING ACROSS THE STRAIT OF JUAN DE FUCA TO THE OLYMPIC MOUNTAINS FROM THE STATION.



A TYPICAL FOREST SCENE NEAR THE STATION. Near the center is a cedar (*Thuya plicata*) with firs (*Abies amabilis*), spruces (*Picea sitchensis*), and hemlock (*Tsuga heterophylla*) near by.

It has been more interesting in other ways than in its crookedness and difficulties. At the port the trees were so tall and large as to attract attention. But as we went deeper into the forest the cedars, spruces, firs, hemlocks and pines became so much larger that we had to stop now and then to admire their giant trunks, and their great masses of green foliage a hundred feet and more above our heads. Huckleberry bushes of two species (*Vaccinium ovalifolium* and *V. parvifolium*) grew by the trail side offering their tempting fruit to us as we passed. There were mosses and lichens everywhere, sometimes hanging from the branches in great masses, a foot in diameter and a yard in length. At our feet, by our side, even on the mossy trunks of the trees, were pretty flowers of many species,

THE GEOLOGIST'S LECTURE IN THE WIND-WORN COVE.

ferns, and even shrubs in profusion. On the ground here and there were gigantic ferns (*Pteris aquilina lanuginosa*) seven to eight feet high, and a yard across, and looking more like shrubs than the modest brakes of the east.

Do you marvel that I call this a wonderful trail, and that in spite of its length and difficulties it was so full of interest that these were soon forgotten, and only its beauties and scientific interest remembered?

The camp, technically known as the Minnesota Seaside Station, consists of a substantially built log living-house and two laboratories, which stand on the shore overlooking the strait of Juan de Fuca and

YOUNG ZOOLOGISTS GETTING SERPULIDS AND STARFISHES.

the Olympic Mountains of Washington. The beach is rocky, and on the rocks and in the water are immense masses of giant seaweeds. Here we are received by the director, a tall, stout man with a twinkle in his eye; the sub-director, a merry little woman, whose specialty is

COLLECTING GIANT KELPS AT LOW TIDE.

the study of the seaweeds, and the chaperon, a cheery old lady, the mother of the sub-director. I look into the big room in the living house and note the great fire-place with a big settee on each side, which promises solid comfort in the cool evenings.

With the director I go to the laboratories, where we find three rooms, in all of which students are hard at work. Here are tables, microscopes, reagents, books and other laboratory apparatus, and the rooms look much like the laboratories in the colleges and universities, except that here the furniture is roughly made by carpenters. We go down to the beach and take a close view of the seaweeds, the hermit crabs, sea anemones and star fishes. We look up and note the gigantic size of the trees which form the forest background. We hear the clangor of a bell, and the director suggests that we hurry back to camp, for that is the noonday dinner signal. He takes us by the men's 'lavatory,' which is a quiet brook near one of the laboratories. Towels and soap are here in profusion, for every man supplies his own. Here, day by day, the men perform their ablutions and make their toilets. The water is always abundant and the toilet room is never overcrowded!

A Vancouver Coast Octopus.

The dinner served in the big room was quite characteristic of our camp life. On each side of two long tables were long plain benches. Over these we stepped to our assigned places. Potatoes, turnips and bacon, with bread, butter and tea, all in generous quantities, constituted the substantial meal. It was a merry meal, as were all our meals. When twenty-two hungry campers sit down to a 'square meal' there is always much jollity. Dinner over, the noonday lecture was announced to take place in a shady spot two hundred yards from the camp. It was given by the director, who sat on a log, and talked to us on the characteristics of the spruce, hemlock and fir trees of the region, while his audience sat on other logs or on the ground near him. Above us are the trees under discussion, and at our feet are the cones which have fallen from them. So we have a bit of out-door laboratory work while listening to the lecture. When we break up, some go to the laboratories, while others stroll over the rocks hunting for specimens.

'JOHN,' THE INDIAN FISHERMAN, BRINGING SALMON; GIANT KELPS (*Nereocystis*) IN THE WATER BEYOND.

INDIAN BASKET-MAKERS; LOW TIDE, EXPOSING ROCKWEEDS (*Fucus*)

Later in the day we take in a quiz given by an instructor to his class in bacteriology. After supper, which is much like dinner, an evening lecture is given by another instructor, who tells us about Agassiz and his work, while we sit about the fireplace, with its crackling fire of driftwood, for the evening is cool, although it is August. A little later I am shown to my berth in the men's compartment on the second floor. The carpenter had built plain wooden berths in tiers of two each, and these have had boughs of cedar laid in them on which I roll in my blankets and sleep soundly. A similar compartment is provided for the women of the party, and here our chaperon gathers her charge. The sounds of the retreating tide as the waves lap upon the rocks lull our senses, and we sink into such a sleep as only those can enjoy who live in camp.

WATCHING THE BLACK-FISH.

The roar of the breakers wakes us early, and I go down to the beach and walk along its rocky ledges for half a mile and watch the swaying kelps (*Nereocystis*) and the nodding sea palms (*Postelsia*) as the waves dash over them. A messenger comes to call me to the breakfast I am forgetting, and we hurry back by a short cut over the neck of a promontory, crossing a canyon on the trunk of a fallen spruce five or six feet in diameter. On its upper side it carries several large trees, and yet its wood is as sound as when it fell a century or so ago. After breakfast the sub-director instructs her class in seaweeds for a couple of hours, when the director takes them and goes

down the beach in search of those giant brown plants of the ocean, called kelps. He leads us a merry scramble, and at last finds what he is looking for (*Dictyoneuron californicum*) and sits on a boulder and gives us an open air lecture upon kelps in general and this one in particular. At its conclusion he announces that we must hurry back in order not to be late to dinner, for promptness in all things is the rule in this school. He who does not appear at meal time doesn't get any meal, that is all. It is a simple rule, and it is very effective.

After the noon meal the geologist takes his class out for their first lecture, finding a rocky, water-worn cove close by the water, where

A July Night on Vancouver Island.

we gather while he tells of the agency of water in rock formation and sculpture, with the cove itself as an example. No method of instruction could be more simple, none more effective. After the lecture we wander for hours over the rocks and study the strata in the cliffs above us. When we get tired of rocks we peer into the tide-pools and study the wonderful vegetation, of a kind all unknown to the inland botanist. Then we poke the spiny sea urchins in their snug niches in the pools, and pester the curious sea anemones, which are to be found everywhere. We watch the crabs, who in turn watch us and scuttle away sidewise with a very knowing look in their funny eyes.

And thus the days go. There is always something a-going. One

evening a mysterious play regarding the 'Hodag' is performed in a lantern-lighted dell in the forest, followed by a sober lecture upon the superstitions of mankind and the difficulties met with in attempting to eradicate them. Another evening a scientific comedy is enacted, followed by a lecture on the structure and activities of the cell, which the play illustrated. Near the close of the session an evening is given to athletic sports, and another to a social dance, in which the music is furnished by a squeaking phonograph.

On the last Sunday evening the geologist gave us an address on 'Science and Religion,' and on the night before camp was broken up the writer hereof gave the closing address on 'The Place of Science in Education.' Yes, we were busy, and learned a great deal not only about the plants and animals and rocks, but many other things as well. The director proved himself a genius in his management of the school and camp, and when we broke up we parted from him with the keenest regret.

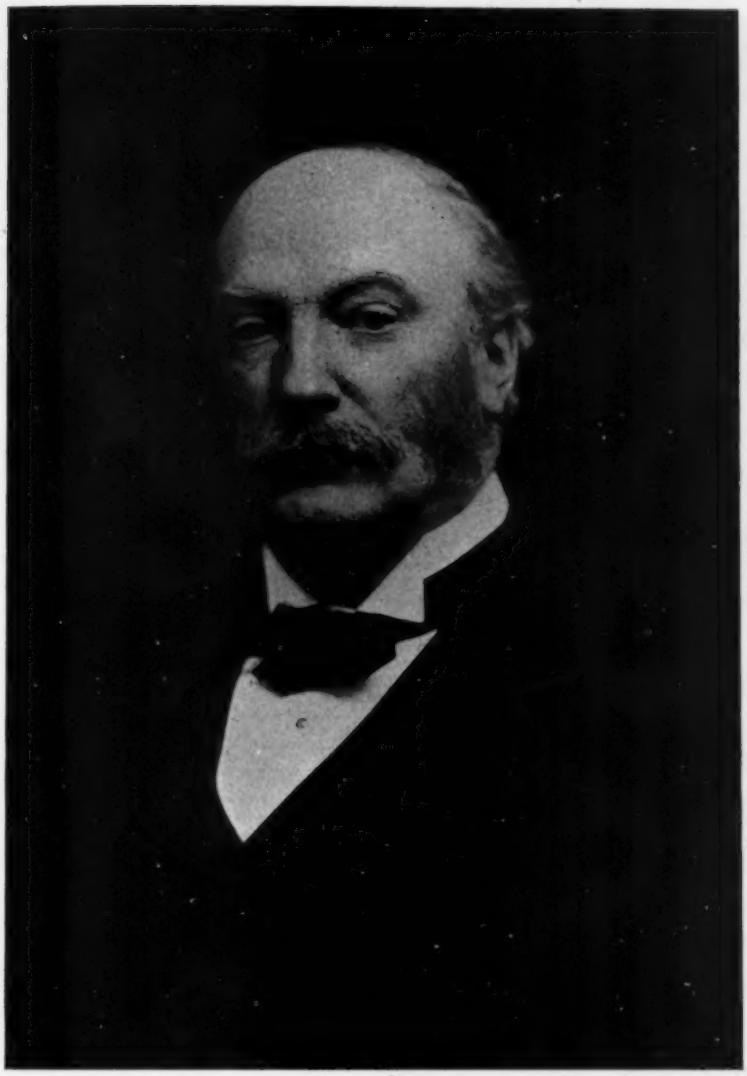
Who goes to such a summer school you ask? That can be answered best by giving a list of those who were there this season. First there were the officials, director, sub-director, chaperon, geologist, doctor and professor. Then the students were a Chicago school teacher, a St. Paul high school teacher of science, three university instructors, two Minneapolis teachers, an Illinois high school teacher of science, six university students and one high school student. The writer may be included here as the guest, making a party of twenty-two in all. I can not think of a more helpful session of combined study and 'outing,' nor of a more natural and effective method of giving and receiving instruction than that in this seaside summer school on Vancouver Island.

THE PROGRESS OF SCIENCE.

THE NOBEL PRIZES.

As has already been noted here the Nobel prizes in science have this year been awarded to Lord Rayleigh, Sir William Ramsay and Professor Ivan Pavlov. Each of these men of science has that international reputation which is said to be the best forecast of the verdict of posterity. Lord Rayleigh and Sir William Ramsay are famous for their joint discovery of argon, to which the latter has added helium, neon, krypton and zenon; but each has long been known as a leader in his science. Lord Rayleigh's great work is his 'Theory of Sound'; his collected papers, recently published, cover a wide range of subjects in mathematical physics. He was Maxwell's successor in the chair of experimental physics at Cambridge, now held by Professor J. J. Thomson, but has since 1884 carried on his researches in his private laboratory in his country place in Essex. Lord Rayleigh is one of those who have given distinction to science in Great Britain without holding a professional position, a class unfortunately lacking in this country. Like Darwin and others of this class, he also represents a hereditary interest in science, his brother having done scientific work, and his son having this year been nominated for membership in the Royal Society. Sir William Ramsay, who has recently been knighted, is professor of chemistry in University College, London. Apart from his discovery of new elements and their properties, he has done important work on the molecular surface energy of liquids and in other directions, including improvements in the teaching of chemistry. His recent visit to America has left most pleasant memories. His address on the 'The Present Problems of Inorganic Chemistry,' given at the St. Louis Congress was published in the issue of this journal for November last. Professor J. P. Pavlov is less well known to Americans than Rayleigh and Ramsay, partly because his researches were originally published in a language difficult to read. His important work on digestion with special reference to the control of the nervous system was translated into German in 1898, and by specialists, at least, it is now fully appreciated. He has also made important improvements in technique and discoveries in regard to the formation of urea, the functions of the liver and in other directions. He is professor in the Imperial Institute for Experimental Medicine at St. Petersburg, which is liberally supported by the Russian government.

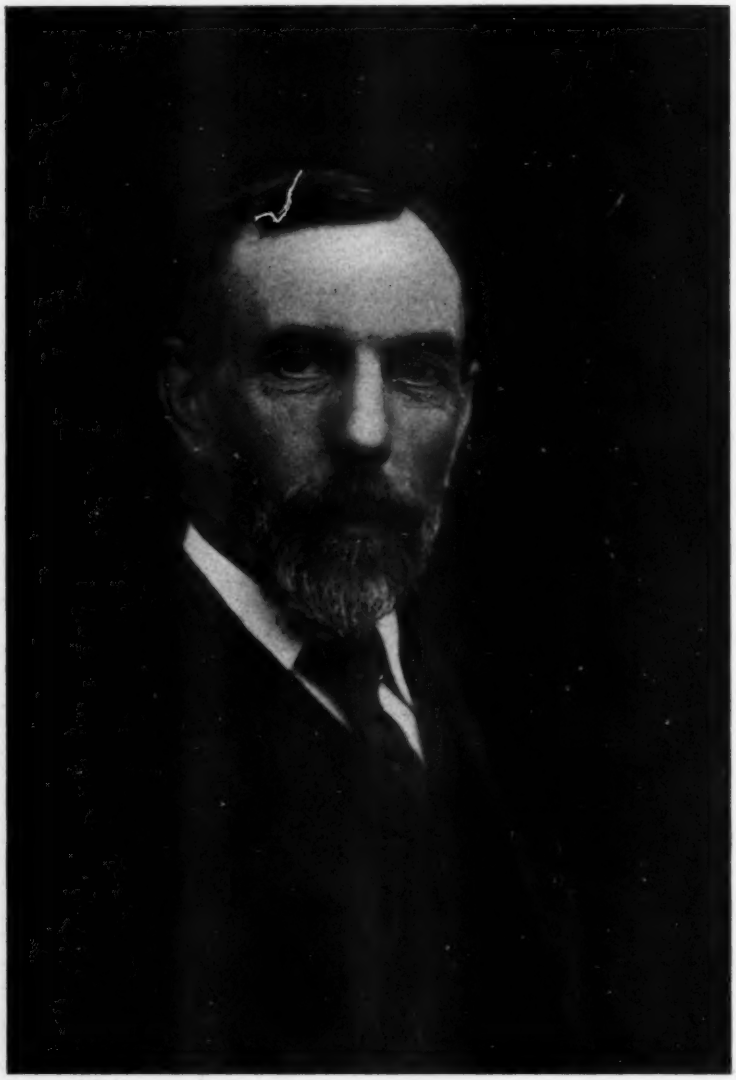
It will be remembered that Alfred Nobel, who amassed a fortune by the invention of dynamite, bequeathed it to form a trust, which amounts to about $8,000,000, "the interest of which shall be distributed annually as a reward to those who, in the course of the preceding year, shall have rendered the greatest services to humanity. The sum total shall be divided into five equal portions, assigned as follows: (1) To the person having made the most important discovery or invention in the department of physical science. (2) To the person having made the most important discovery or having produced the greatest improvement in chemistry. (3) To the author of the most important discovery in the department of physiology or of medicine. (4) To the author having produced the most notable literary work in the



LORD RAYLEIGH.

sense of idealism. (5) To the person having done the most, or the best, in the work of establishing the brotherhood of nations, for the suppression or reduction of standing armies, as well as for the formation and the propaganda of peace conferences."

It appears that the explicit directions of the founder have been violated in at least two ways. Certain local Nobel institutes have been established at Stockholm, on which about a third of the income is spent, and a prize has in no case been awarded to a man for work done or made known 'in the course of the preceding year.' It would be unfortunate to violate a trust of this kind, even though its provisions were unwise; as a matter of fact the instructions of Nobel were better considered



SIR WILLIAM RAMSAY.

than the substitute. Very large prizes, say $50,000 each, for the best scientific work of the year, given often to young men, would more directly stimulate research and would be more useful to the recipients than prizes given for general eminence.

The prizes in the sciences have been awarded as follows:

	PHYSICS.	CHEMISTRY.	MEDICINE.
1901.	Röntgen.	Van't Hoff.	Behring.
1902.	Lorentz and Zeeman.	Fischer.	Ross.
1903.	Becquerel, M. and Mme. Curie.	Arrhenius.	Finsen.
1904.	Rayleigh.	Ramsay.	Pavlov.

The prizes have gone three times to England and to Germany; twice to Holland (though Van't Hoff is now in

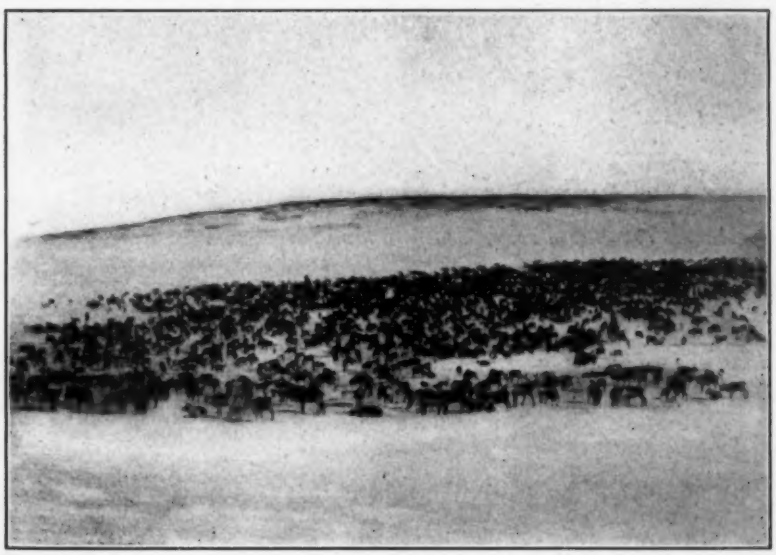
Berlin); once to France, to Sweden, to Denmark and to Russia.

The Independent raises the question as to whether America has been neglected, and asks for a vote for five Americans who deserve the prize next year. It must unfortunately be acknowledged that no American deserves the prizes in science on the lines followed by the administrators of the trust. Nobel himself laid special stress on an invention benefiting mankind, and Dr. Bell and Mr. Edison are here preeminent. In pure science Willard Gibbs deserved the prize, but we have now no physicist, chemist or physiologist as eminent as Europeans who could be named. We stand better in some other sciences, and we may hope in work being carried forward by men who may ultimately attain international eminence.

THE JESUP NORTH PACIFIC EXPEDITION.

Mr. Morris K. Jesup, president of the American Museum of Natural History, provided in 1897 means for a thorough ethnological exploration of the northern coast of North America and Asia from British Columbia to the Amur River. The expedition has resulted in valuable accessions to the American Museum and in important ethnological, archeological, linguistic and anthropometric studies, which are now in course of publication in twelve volumes under the editorship of Dr. Franz Boas, of the American Museum and Columbia University. Investigations along the northern coast have been carried out by Messrs. Boas, Farrand, Smith and Teit, and on the Asiatic side by Messrs. Laufer, Fowke, Jochelson and Bogoras.

The memoir last published is an account of the material culture of the Chukchee, the tribes inhabiting the extreme northeastern corner of Siberia, of special interest consequently to those who speculate on the past peopling of the Americas by way of Bering Strait or the future construction of telegraphs and railways through these regions. It also has an adventitious interest just now in view of a possible change in sovereignty. It appears, indeed, that this territory has not been completely



Reindeer Herd.

REINDEER CHUKCHEE YOUNG MAN.

ESKIMO GIRL.

MARITIME CHUKCHEE WOMAN.

REINDEER CHUKCHEE WOMAN.

subdued, the Russians having withdrawn in 1764, leaving the inhabitants to settle their affairs according to their own customs. Commerce has in part accomplished what force failed to do, though the bulk of the territory remains exempt from any trace of Russianization.

The Chukchee number only about 12,000, of whom about one quarter are maritime and three quarters are reindeer people, while there are about 1,200 Eskimo on the coast. It is not settled as to whether the Chukchee and Eskimo belong to the same stock. Types of Mongolian faces are not uncommon, and at present there is a good deal of admixture. The domestication of reindeer is characteristic of the tribes inhabiting the Asiatic side of Bering Sea, and their economic condition resembles that of the more southerly cattle-breeding tribes. The large size of some of their herds is shown in the illustration. The Chukchee depend on reindeer for clothing and for food; for the

covering of their huts and for transportation. Great numbers of these animals are annually slaughtered, Mr. Bogoras having used as many as fifty in one month as food for his dogs. The price of a reindeer is a package of brick tea and a bundle of tobacco, the value of which is together about a dollar. The maritime Chukchee engage principally in fishing and there is a good deal of exchange between them and the reindeer people.

The houses have a wooden frame covered with skins, sometimes protected as shown in the illustration, by sods or stones. The diameter of the hut or tent is from fifteen to twenty-five feet, and there is an inside room usually about 4 ft. 6 in. in height, 7 ft. in breadth and 12 ft. in length. Fifty or more reindeer skins are used to cover one of the huts, and they require a great deal of care. New skins are used in winter and old skins in summer. Fire is used for cooking, but scarcely heats the house beyond the temperature of the outside air. The inner compartment is lighted by a lamp, but is heated chiefly by the bodies of the inhabitants. As the inner room is used not only for sleeping, but also for eating and entertaining, every square foot is occupied.

The maritime Chukchee eat chiefly the meat of sea-mammals, while the others depend on the reindeer. They like raw frozen meat, and do not object to its being putrid. They drink tea and smoke tobacco continually, and use as much alcohol, which may be unrectified and undiluted 95 per cent. spirit, as they can get.

Fly-agaric is the only means of intoxication discovered by the natives of northeastern Asia. It is made from mushrooms and appears to produce effects similar to hasheesh. The sense of smell of the natives is said to be very acute; their color nomenclature is defective, which may be due to defective vision or to lack of interest in colors.

SCIENTIFIC ITEMS.

SENHOR MANUEL GARCIA celebrated his hundredth birthday on March 7 in excellent health. He gave the first performance of Italian opera in New York City in 1825, and was long celebrated as a teacher of singing. His important

CHUKCHEE WINTER HOUSE.

contribution to science was the invention of the laryngoscope fifty years ago. Senhor Garcia was presented with a portrait of himself by Mr. John S. Sargent and received a number of congratulatory addresses with decorations from King Edward, Emperor William and the king of Spain.

THE first John Fritz gold medal will be conferred upon Lord Kelvin. This medal is awarded by a joint committee of the American Institute of Electical Engineers, the American Society of Mechanical Engineers, The American Society of Civil Engineers and the American Institute of Mining Engineers to the man most representative of, and eminent in, scientific advance in the engineering field.—A medal in commemoration of M. Alfred Cornu will be struck by the French Physical Society.—It is proposed to erect a memorial in Jena to Professor Ernst Abbe in commemoration of his services to optical science and industry.

THE first Herbert Spencer lecture, established by Pandit Shyamâji Krishnavarma, M.A., of Balliol College, was given at Oxford on March 9, by Mr. Frederic Harrison, M.A., honorary fellow of Wadham College.—Dr. H. A. Lorentz, professor of physics in the University of Leyden, and Professor V. F. Bjerknes, professor of mathematical physics in the University of Stockholm, will give courses of lectures at Columbia University next year.

DR. LEWELLYS F. BARKER, professor of anatomy at the University of Chicago, has been called to the chair of medicine in Johns Hopkins University, vacant by the removal of Dr. William Osler to Oxford. At the same time Dr. W. S. Thayer, associate professor of medicine, has been advanced to a professorship of clinical medicine.—Dr. Frank Schlesinger, of the Yerkes Observatory, has been elected director of the New Allegheny Observatory in succession to Dr. F. L. O. Wadsworth, who has resigned.